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November 27, 1843.

On the Foundation of Algebra, No. III. By Augustus De Morgan, of Trinity College, Professor of Mathematics in University College, London, &c.

In the second paper of this series a general definition of the operation A^B was laid down, A and B being each of them any form of $p + q\sqrt{-1}$. The logarithm (or as Mr. De Morgan calls it, the *logometer*) of a line is thus described:—a line whose projection on the unit-axis is the logarithm of the length, and whose projection on the perpendicular is the angle made with the unit-axis (or its arc to a radius unity). Thus a line r inclined at an angle θ has for its logometer a line $\sqrt{(\log^2 r + \theta^2)}$ inclined at an angle whose tangent is $\theta : \log r$. This being premised, the universal definition of A^B is the line whose logometer is $B \times$ logom. A.

The object of this third paper is to show that the preceding definition of the logometer is not the most general. Take any two lines whatsoever passing through the origin, and style them the bases of length and direction. Set off on the first a line representing the logarithm of the length in question, and on the second a line representing the angle it makes with the unit-axis, both on any scale of representation. Then the diagonal of the parallelogram described on the lines just set off is a logometer to the length and direction from which it was derived; and if under this meaning of the word logometer the preceding definition of A^B be employed, the equations

$$A^B A^C = A^{B+C}, \quad A^B C^B = (AC)^B, \quad (A^B)^C = A^{BC}$$

are universally true.

There is no necessity for the introduction of this more general system, since all its results can be expressed in terms of those of the more simple definition in the second paper. This new definition of the logometer is really nothing more than the process answering to the extension of the theory of logarithms from the system constructed on the Naperian base, to that which is on any base whatsoever.

On the Measure of the Force of Testimony in cases of Legal Evidence. By John Tozer, Esq., M.A., Barrister-at-Law, Fellow of Gonville and Caius College.

The object of this paper is to show that the assumptions made by some English legal authorities on this subject, in opposition to the principles established by scientific processes, are not justified.

The views more particularly dissented from, as extracted from a work of high legal authority, are thus enunciated:—

"The notions of those who have supposed that mere moral probabilities or relations could ever be represented by numbers or space, and thus be subjected to arithmetical analysis, cannot but be regarded as visionary and chimerical.

"Whenever the probability is of a definite and limited nature (whether in the proportion of one hundred to one or of one thousand to one, is immaterial), it cannot be safely made the ground of conviction; for to act upon it in any case would be to decide, that for the sake of convicting many criminals the life of one innocent man might be sacrificed.

"The distinction between evidence of a conclusive tendency which is sufficient for the purpose, and that which is inconclusive, appears to be this: the latter is limited and concluded by some degree or other of finite probability beyond which it cannot go; the former, though not demonstrative, is attended with a degree of probability of an indefinite and unlimited nature."

The method pursued is that of investigating algebraic expressions for the probabilities that the allegations made in a case which actually occurred, the trial of a female for murder, are true; and thence deducing an expression for the probability of the truth of the charge, in passing from the symbolical to the numerical expression, the numbers employed are not the actual values of the symbols but their limiting values; the resulting number is therefore a fraction which is not less than the value of the probability of the truth of the principal allegation, this being what in practice is required.

The conclusion arrived at is, that the mode of estimating the force of evidence employed in a court is a process which algebraic investigation analyses, and of which it explains the theory, and an approximation to a result which is obtained with accuracy by assigning numerical values to the algebraic symbols: a clear conception of the nature of the practical process, it is conceived, must render its application more accurate, and to the extent of affording this the investigation is deemed to be of practical utility.

December 11, 1843.

On the Motion of Glaciers. By William Hopkins, M.A., F.R.S., Fellow of the Society (Second Memoir).

In a previous memoir Mr. Hopkins had given the details of certain experiments, by which it was proved that ice will descend with

a very slow unaccelerated motion down an inclined plane, presenting a surface like that of a common slab of paving-stone, at an angle scarcely exceeding half a degree (and probably also at still smaller angles), provided the lower surface of the ice in immediate contact with the inclined plane be in a state of constant but slow disintegration. This experimental conclusion was brought forward in support of the *sliding theory* of De Saussure, and the author endeavoured to explain, according to that theory, different phænomena connected with the motion of glaciers. He there considered glacial ice as a *solid* substance, having a certain degree of *plasticity* and *flexibility*, and the general mass of the glacier as a *dislocated* mass, the greater motion of the central portion of the glacier being much facilitated by these dislocations, though due partly, but in a comparatively small degree, to the plasticity of the general mass. In the present memoir Mr. Hopkins considers what would be the nature of the motion under other hypotheses respecting the constitution of glaciers. (1.) The lower part of a glacier may be conceived to be crushed, and consequently disintegrated, by the superincumbent weight, each component particle still retaining its solidity; or (2.) the whole mass may be conceived to be *plastic*, and to move by a change of form, produced by gravity, in each component element. The author contends, if either of these hypotheses were true, that *cæteris paribus*, the more superficial portion of the mass must tend to move the faster as the depth of the glacier should be greater; and that, consequently, the part of the glacier near the upper extremity must generally tend to move much faster than that near the lower extremity, assuming always the whole, or much the greater part of glacial motion, to be due to the plasticity of the mass, and to be independent of *sliding* over its bed. But in such case it is manifest that the general state of a glacier must be one of *longitudinal compression*, more particularly during the summer months, when the motion is greatest. Now the author contends that the general existence of transverse fissures (at least during summer) is a conclusive proof against the existence of general longitudinal compression; and he observes that no observer ventured to assert the fact of such compression to be deducible from actual observation. He conceives this to be a serious objection to the hypothesis here considered.

In this memoir Mr. Hopkins has also investigated the directions in which transverse fissures must be formed when referrible to the internal tensions superinduced by the conditions to which glaciers in general are subjected, and more especially by the more rapid motion of their central portions.

Assuming the velocity of each particle of the glacier to be the same in any vertical line (which is at least true at points not remote from the surface), the glacier may, in this investigation, be considered as a *lamina*. In this lamina take a rectangular element having two of its sides parallel to the axis of the glacier, and, therefore, the remaining sides perpendicular to it. Let X denote the intensity of the force acting *normally* to these latter sides of the element, Y that of the force acting normally to the two former sides. Also let f de-

note the intensity of the force acting *tangentially* on the sides on which X acts normally. It is proved that f will also be the intensity of the tangential force on the other two sides. Then, if θ be the angle which the line of maximum tension through the proposed element makes with the axis of the glacier, it is proved that

$$\tan 2\theta = \frac{2f}{X - Y},$$

where X and Y are *tensions*. If either be a *pressure*, it must be made negative.

If the maximum tension become greater than the cohesion of the ice, a fissure will be formed in a direction perpendicular to that of the tension at each point, or at least approximately so. Consequently, the line whose direction is defined by the angle θ, will be a normal to the curve of fracture. Now, taking the case in which the glacial valley contracts in descending (which is the more frequent case), Y is doubtless most frequently a *pressure*, in which case

$$\tan 2\theta = \frac{2f}{X + Y};$$

also f will be greatest at the sides (where the velocities of particles in a transverse line vary most rapidly), and will vanish at the centre. Hence θ will vanish at the centre of the glacier, and will increase towards the sides, since the change in the value of the denominator cannot be great. Consequently, if a fissure were continued across the glacier it would form a curve, meeting the axis of the glacier at right angles; and its convexity will be turned towards the upper extremity of the glacier, for the line defined by the angle θ, or the normal to the curve, meets the axis of the glacier when produced towards its *lower* extremity. This is the well-known character of transverse fissures, which the author conceives to be thus completely accounted for. In the previous memoir above referred to, this curious character had been very imperfectly explained by referring it to the action of the longitudinal tension (X) alone.

In conclusion the author has replied to the objections against the *sliding theory* urged by Prof. Forbes and others.

February 5, 1844.

On the Fundamental Antithesis of Philosophy. By W. Whewell, D.D.

The fundamental antithesis here spoken of, is that which is variously expressed by the opposition of thoughts and things, theory and fact, ideas and senses, necessary and experimental truth; also by the opposition of reflection and sensation, subject and object. It is remarked that we can have no knowledge without the union, no philosophy without the separation of these two elements. This fundamental antithesis of philosophy is an antithesis of inseparable ele-

ments. It is also shown that the terms which denote the two elements of this antithesis cannot in any case be applied absolutely and exclusively. We cannot say, this is a fact and not a theory, or this is a theory and not a fact; for a true theory is a fact; a fact is a familiar theory. It was further observed, that the antithesis being inseparable, one element seems, and is asserted, in different systems of philosophy, to be derived from the other; ideas from experience, or experience from ideas. But we must always have both elements: thus in mechanics, and in our experience, we have necessary principles, such as that every event must have a cause; and in chemistry also other necessary principles, as that the chemical composition of a body determines its kind and properties.

March 4, 1844.

On the Method of Least Squares. By R. L. Ellis, Esq.

The aim of this paper is to give a succinct exposition of the different demonstrations by which it has been proposed to establish the validity of the rule known as the method of least squares. The first demonstration of this celebrated rule (which had been previously proposed by Legendre) is that given by Gauss in the *Theoria Motûs*. The next appears to be that of Laplace, which has been followed, without variation of principle, by Poisson and other French writers. The demonstration of Gauss is based upon the assumption, that the arithmetical mean is the most probable result to be derived from a series of direct observations of an unknown magnitude. This assumption is alleged by Laplace to be altogether precarious; and it appears that Gauss acquiesced in this remark, as he subsequently, in the *Theoria Combinationis Observationum*, produced another demonstration, which is independent of this assumption. As the first method of Gauss has been followed by later writers, of whom Encke is one, it seemed desirable to endeavour to ascertain if the objection of Laplace be well-founded; and this the writer has attempted to do in the first part of the present communication. His conclusion is, that although the practice of adopting the arithmetical mean as an approximation to the true value of the unknown magnitude observed, is founded on just principles, yet that we are not entitled to say that it leads to the most *probable* result; and consequently that the demonstration in question is invalid.

The writer then proceeds to consider Laplace's demonstration. This involves no precarious assumption, but the mathematical part of the investigation is of very considerable difficulty, and cannot be said to be altogether free from doubt. For Laplace's analysis, another, founded on a theorem which was first made use of by Fourier in his researches on heat, is substituted; and by this change the mathematical difficulties of the subject are very much diminished. An attempt is also made to test the accuracy of Laplace's methods by reference to a particular case.

The third part of the paper relates to Gauss's second method. The relation in which it stands to that of Laplace is distinctly pointed out; the difference between them arising merely from this, that whereas Laplace considered the importance of a positive or negative error (that is, of an error in excess or defect) to be proportional to its arithmetical magnitude, Gauss assumes the square of the magnitude of the error as the measure of its importance, alleging that this importance not being a magnitude, does not strictly admit of numerical evaluation; that some assumption is therefore requisite, and that that which he proposes is not more arbitrary than Laplace's, while, from the absence of discontinuity, it leads to far simpler and more satisfactory calculations.

The writer then shows that neither Laplace's investigations, nor that of Gauss in the *Theoria Combinationis Observationum*, tends to prove, that the results of the method of least squares are the most *probable* of all possible results. This point, with regard to which there has occasionally been some degree of confusion, seems to be essential to a just apprehension of the nature of the subject. It may be remarked, with reference to it, that Laplace uniformly speaks of the method of least squares as the most *advantageous* method of combining discordant observations, or as that which gives the most *advantageous* results, and never as a method by which the most probable results are to be obtained.

Lastly, the writer proceeds to consider three demonstrations of the method of least squares, given by Mr. Ivory in the Philosophical Magazine. These demonstrations are independent of the theory of probabilities. The first is founded upon an assumed analogy between the equilibrium of weights on a lever and the combination of discordant observations; the second upon another unsatisfactory analogy; and the third upon the principle that the error committed at one observation is independent of that committed at any other. None of these demonstrations appear to the writer to be at all conclusive, but they seemed to deserve consideration, not only from the high reputation of their author, but also from the terms in which they have been mentioned in a recent work on the theory of probabilities.

On Divergent Series, and various Points of Analysis connected with them. By Augustus De Morgan, Esq.

The author states that he does not pretend to have perfect confidence even in convergent series. It is the main object of his paper to show that the continental analysts are not justified in their rejection of some classes of divergency, and retention of others, by anything but experience; that they have underrated the character of most which they reject, and overrated that of all they receive.

Divergent series are either of *infinite* divergence, such as $1 \pm 2 + 3 \pm 4 +$ &c., in which summation of terms may give any sum, however great; or of *finite* divergence, such as $\cos\theta + \cos 2\theta + \ldots$, in which no number of terms can give more than a certain quantity. The former are rejected by most modern continental writers, the latter are retained.

Section 1.—All divergent series, whether their divergence be finite or infinite, stand upon the same basis, and ought to be accepted or rejected together, as far as any grounds of confidence are concerned which are not directly derived from experience. The author examines the reasons which Poisson gives for maintaining the contradictory of the preceding. That great analyst considers $1-1+1-1+\ldots$ for instance, as $1-g+g^2-g^3+\ldots$, where g is less than unity by an infinitely small quantity. Mr. De Morgan maintains, that this method, if allowed in transformation of a finite diverging series into a convergent one, of which the convergency only begins after an infinite number of terms, must also be allowed, unless reason can be shown against it, in the destruction of the infinite character of an infinitely diverging series, by the tacit retention of the infinite remainder after an infinite number of terms.

The author would not use any series, so as to place absolute dependence upon their results, unless the producing functions were known: and this because series themselves neither show discontinuity nor infinity, when it takes place; and because it happens that divergent series, at least, and perhaps others, may represent one thing or another, according to the general form of which they are made particular cases.

Mr. De Morgan observes that a divergent series, which is not considered as arithmetically infinite, such as $1+2+4+\ldots$ may be so in reality, in particular cases. This series being called S, satisfies the equation $S=1+2S$, and this gives $S=-1$, the usual value of the series. But it is to be remembered that an equation may be a degenerate case of an equation of higher degree, in which case it has one or more roots infinite. An instance is produced in which $1+2+4+\ldots$ certainly represents infinity.

Finally, the author remarks that there is much more safety in series with terms alternately positive and negative, whether their divergence be finite or infinite, than in series of finite divergence, as such.

Section 2.—The operation of integration, as at present understood, is one of arithmetic, as distinguished from algebra, and must not be applied unreservedly to divergent series. The author supports the first part of this assertion upon the circumstance that the only definition of integration which is generally applicable is the *summatory* one, in which $\int \phi x \, dx$ does not mean the function whose differential coefficient is ϕx, but the limit of the summation expressed by $\Sigma(\phi x \Delta x)$. He then goes on to show instances in which it is unquestionably not allowable to apply integration to infinitely divergent series: and he asserts throughout the paper generally, that all the instances in which error has been shown to arise from the use of infinitely divergent series, have been those in which integration has been applied, and those only.

In this section warning is also given against the supposition that $0+0+0+\ldots$ must represent 0 in all cases.

Section 3.—It generally happens that the real analytical equivalent of the different values of an indeterminate expression, is the mean

of these different values. This principle is the one which was adopted by Leibnitz in his well-known explanation of the meaning of $1-1+1-1+\ldots$ Without assuming that anything like proof can be given, the author notes, as instances in which the thing asserted is true, algebraical series, trigonometrical series, Fourier's integral, Poisson's expression of a function between any limits by an infinite series of trigonometrical integrals, and also the sine and cosine of *infinity*. Assuming $\int_a^b \phi x \, dx : (b-a)$ to represent the mean value of ϕx between a and b, the author tries what ought to be the value of $\tan \infty$, if this principle be true, and finds $\pm \sqrt{-1}$, which on trial is found to satisfy the fundamental equations of trigonometry.

Section 4.—Series of alternately positive and negative signs stand upon a much safer basis than those in which all the terms have the same signs, and that whether their divergence be finite or infinite.

It has long been observed, that when the terms of an alternating series begin by diminishing, even though they afterwards increase, the converging portion may be made effective in approximating to the arithmetical equivalent of the series. The error committed by stopping at any term is not so great as the first of the rejected terms. In many alternating series this has been proved to be true, and it seems never to have been supposed that the theorem was anything but universal. In this section instances are produced in which the theorem is not true; and at the same time various proofs of it are given, each of which applies to very extensive cases, and the tendency of which is to show that it is only under definite and unusual conditions that the theorem can fail. Still, however, no positive criterion is established for ascertaining whether the theorem be true or not in any particular case.

Section 5.—On double infinite series, in which the terms are infinitely continued in both directions.

It seems, in many different ways, that the series

$$\ldots + \phi(x-2) + \phi(x-1) + \phi x + \phi(x+1) + \phi(x+2) + \ldots$$

can be resolved, by analytical transformation, into $0+0+0+0+\ldots$ When there is no discontinuity whatever in the relation between $\phi x + \phi(x+1) + \ldots$ the value of the preceding is 0. But when discontinuity does exist, the value of the series may be some other solution of $\psi(x+1) = \psi x$. This assertion, derived from observation of instances, is here discussed in the case of

$$\phi x = \frac{1}{1+(b+cx)^2};$$

the value of the double series is obtained, and some corresponding products of an infinite number of factors are deduced.

April 20, 1844.

On the Transport of Erratic Blocks. By W. Hopkins, M.A., F.R.S. &c.

The principal object of this paper is to investigate the transporting power of currents of water in general, and to explain in particular the nature of those which would arise from the instantaneous or paroxysmal elevation of any considerable extent of the earth's surface lying beneath the surface of the sea. The author has termed them *elevation currents.* The immediate effect of an elevation like that just supposed, would be the elevation to a nearly equal height, of the surface of the superincumbent water, whence a great wave would diverge in all directions. Such a wave would be attended by a *current* in the direction of the wave's propagation, and has thence been called a *wave of translation*. When such a wave proceeds along a uniform canal, Mr. Russell has established experimentally the following facts:—

1. Every particle in the same transverse section of the canal has the same motion.

2. The velocity with which the wave is propagated is equal to that due to half the height of the crest of the wave above the bottom of the canal.

From these data the author has calculated the velocity of the currents which would necessarily attend these waves of elevation. It depends principally on the height of the elevation and the depth of the sea, while the time during which the current will flow depends principally on the extent of the elevated area and the depth of the sea. Thus if the depth of the sea should be 300 feet, and the height of the crest of the wave above the even surface of the sea (which may be considered as approximately the same as the elevation of the suddenly raised area) should be 50 feet, the wave would be propagated with a velocity of upwards of 70 miles an hour, and the attendant current would be upwards of 10 miles an hour. Also, if the elevated area were circular, the width of the wave would exceed the radius of the circle. The wave would have the essential character of a tidal wave termed a *bore*, except that it diverges in all directions, instead of proceeding along a confined channel.

The author next proceeds to calculate the motive power of currents of water. Let v be the velocity of the current, ρ_1 the density of the water, and S the area of a plane surface on which the current acts, and so placed as to make an angle θ with the direction of the current; then if R denote the whole normal action of the current on S, we have

$$R = \frac{v^2}{2}\rho_1 S \sin^2\theta,$$

provided θ do not deviate too much from 90°. When $\theta = 90°$ the truth of this formula has been proved by numerous experiments, for all velocities up to 11 or 12 miles an hour, and may be assumed to

hold, at least approximately, for still greater velocities. It has also been proved experimentally to be approximately true for any value of θ not differing by more than 45° from a right angle, as is the case in the applications made of the formula.

The velocity of the current just sufficient to move a block will depend on the volume, the specific gravity, and the form of the block. If the block *slide*, much will depend on the nature of the surface over which it is transported, and thus a very uncertain element will be introduced into the calculations. This uncertainty, however, will be in a great degree removed if we calculate the force sufficient to make the block *roll*. Each block would present a separate problem if it were required to find accurately the current necessary to move it, but as great accuracy is not necessary in the cases here contemplated, it is sufficient to make the calculations for a few determinate and simple forms as those to which more irregular forms may be referred with a sufficient approximation to accuracy. Thus the author has considered the cases of blocks whose sections perpendicular to their length are squares, pentagons, hexagons, &c., and has calculated their dimensions, that a current of about 10 miles an hour might just be sufficient to make them move by rolling. Assuming the specific gravity of the blocks to be 2·5, we have the following results:—

1. A *parallelopiped.*

Side of the square section perpendicular to its length = 2·73 feet.

2. A *pentagonal prism.*

Side of the pentagonal section perpendicular to its length = 2·27 feet.

3. A *hexagonal prism.*

Side of hexagonal section perpendicular to its length = 2·3 feet.

When the motion takes place, as here supposed, in a direction perpendicular to the length of the block, the efficiency of the current to move it will evidently be independent of the length of the block. If we suppose the length of the parallelopiped to be equal to the side of a section of it taken as above, it becomes a cube; and if we take the lengths of the blocks in the other two cases to be equal to twice the length of the sides of their sections respectively, their lengths will not much exceed their heights. Then the weights of the blocks will be $1\frac{1}{2}$ ton in the first, nearly 3 tons in the second, and upwards of 4 tons in the third case. Again, if the block be an oblate spheroid resting with its axis vertical, and the polar axis $= \frac{3}{4}$ths of the equatorial diameter, the current of about 10 miles an hour will just make it roll if its height be about 2 feet, and its weight about 4 tons. If the polar axis $= \frac{5}{6}$ths of the equatorial diameter, the block will be just moved, provided its height be $3\frac{1}{3}$ feet and its weight 14 or 15 tons.

In this part of the investigation it is shown that the power of rapid currents to transport blocks of enormous magnitude is perfectly consistent with the almost inappreciable power of currents of which the velocity does not exceed, for instance, 2 miles an hour; for it is shown that *the weight of a block of given form and specific*

gravity, which may thus be moved, varies as the 6th power of the velocity of the current. Thus if a current of 10 miles an hour will just move a block of a certain form, whose weight is 5 tons, a current of 15 miles an hour would move a block of similar form of upwards of 55 tons. A current of 20 miles an hour would, according to the same law, move a block of 320 tons, while a current of 2 miles an hour would scarcely move a small pebble.

In the previous calculations the relation between the magnitude of the block and the velocity of the current has been determined on the supposition that the current, at the instant it acquires its greatest velocity, shall just be able to move the block, which would again be left at rest without being moved through any sensible space. If the velocity be greater or the mass smaller, the block will be transported to a distance which the author has calculated. Let

v_2 be the velocity of a current just sufficient to move an assigned block;
v_1 the velocity of the transporting current acting on the above block, v_1 being greater than v_2;
l the breadth of the great wave of translation producing the current;
h the height of the highest point of the wave above the level of the ocean;
H the depth of the ocean;
s the space through which the block is transported by the wave.

The following Table gives corresponding values of these quanti-

H.	h.	V.	v_1.	v_2.	s.	s_0.
feet.	feet.	miles.	miles.	miles.		
200	50	62	12	5	$\frac{l}{30}$ nearly	$\frac{l}{10}$ nearly
				10	$\frac{l}{372}$ ...	
300	50	73	10·4	5	$\frac{l}{52}$ ...	$\frac{l}{14}$ ...
300	100	77	19·4	5	$\frac{l}{14}$ ...	$\frac{l}{8}$ ...
				10	$\frac{l}{34}$ ...	
400	100	86	17	5	$\frac{l}{20}$ ...	$\frac{l}{10}$ ...
				10	$\frac{l}{60}$ ...	
450	150	95	23·5	10	$\frac{l}{24}$ ...	$\frac{l}{8}$ ...
600	150	106	20·5	10	$\frac{l}{39}$ ...	$\frac{l}{10}$ ...
800	200	140	28	10	$\frac{l}{25}$ ...	$\frac{l}{10}$ ...

ties. The last column gives the corresponding value of the space (s_0) through which a particle of the water, or any body floating in the water, will be carried by the wave. The expressions for s and s_0 are

$$s = \frac{1}{2} \cdot \frac{(v_1 - v_2)^2}{V\, v_1}\, l,$$

$$s_0 = \frac{1}{2} \cdot \frac{v_1}{V} \cdot l,$$

V being much greater than v_1. [See preceding page.]

In estimating the magnitude of a block which may be moved by a given current, the transport is supposed to take place over a horizontal surface sufficiently hard and even for the block to roll upon it without impediment. In other states of the surface the transport might be more or less impeded. The constant action of denuding causes would be highly favourable to the transport by the successive removal of local impediments. The author conceives that the objection to this mode of transport, founded on inequalities of surface which now exist between the original site of a block and its present position, have been far too much insisted on by some geologists, for, he contends, such inequalities could not generally exist under the continued action of denuding causes, among the most powerful of which may be reckoned the transporting currents themselves.

It should be remarked, that it appears from the values of s given in the preceding table, that the space through which any considerable block could be moved by a single wave of elevation, is only equal to a small fraction of the breadth of the wave. Consequently, if such a block has been moved by this agency to a considerable distance from its original site, the transport must have been effected by a repetition of transporting waves; and, therefore, since a wave of considerable height can only be produced by a *sudden* elevation, this theory of transport is ultimately associated with the theory which attributes the more marked phænomena of geological elevation to a repetition of *paroxysmal* movements.

The author concludes with some general observations on the evidence by which we may hope to distinguish between the effects of the three different agencies to which the transport of blocks may be attributed—glaciers, floating ice, and currents of water. Large angular blocks in the immediate neighbourhood of glacial mountains (such as the alpine blocks) may doubtless, in many cases, be referred to glaciers, while the transport of similar blocks to great distances may be referred to floating ice. Smooth rounded blocks of smaller dimensions, especially when spread out with other detrital matter in layers of considerable horizontal extent, the author would refer to the action of aqueous currents.

PROCEEDINGS

OF THE

CAMBRIDGE PHILOSOPHICAL SOCIETY.

October 28, 1844.

On the Foundation of Algebra, No. IV.—On Triple Algebra. By Augustus De Morgan, Esq., of Trinity College.

The extensions which have successively been made in algebraical interpretation have been consequences of efforts to interpret symbols which *presented themselves* as necessary parts of the algebraical language which is suggested by arithmetic. The now well-known signification of $a+b\sqrt{-1}$ did not yield any new imaginary or unexplained quantities: and accordingly no effort (within the author's knowledge) was made to produce an algebra which should require three dimensions of space for its interpretation, until Sir William Rowan Hamilton wrote a paper (the first part of which appeared in the Philosophical Magazine* before the present one was begun) on a System of Quaternions. This system, as the name imports, involves four distinct species of units, one of which may by analogy be called *real*, the three others being *imaginaries*, as distinct from one another as the imaginary of ordinary algebra is from the real. These imaginaries are not deductions, but inventions; *their laws of action on each other are assigned*: this idea Mr. De Morgan desires to acknowledge as entirely borrowed from Sir William Hamilton.

Sir William Hamilton has rejected the idea of producing a triple algebra, apparently on account of the impossibility of forming one in which such a symbol as $a\xi+b\eta+c\zeta$ represents a line of the length $\sqrt{(a^2+b^2+c^2)}$. Mr. De Morgan does not admit the necessity of having a symmetrical function of a, b, c, and, throwing away this stipulation, points out a variety of triple systems, partially or wholly interpreted.

Sir William Hamilton's quaternion algebra is not entirely the same in its symbolical rules as the ordinary algebra: differing in that the equation $AB=BA$ is discarded and $AB=-BA$ supplies its place. Those of Mr. De Morgan's system, which are imperfect, all give $AB=BA$, but none of them give $A(BC)=(AB)C$, except in particular cases.

* Vol. xxv. pp. 10, 241.

Mr. De Morgan gives systems of triple algebra, which he distinguishes into quadratic, cubic, and biquadratic, according as the invented imaginary units represent square roots, cube roots, or square and fourth roots, of the negative real unit. It would not be easy in an abstract to give any account of these, but among them are found,—

1. An imperfect quadratic system, strongly resembling the common double algebra, and which would, but for its imperfect character, be at once recognised as the proper and natural extension of the interpretation of imaginary quantities to three dimensions of space: the ultimate symbol for a line is $l(\cos\theta+\sin\theta\sqrt{_\omega -1})$.

2. An imperfect quadratic system, very like the former one, except in having a peculiar inversion in the operation of multiplication, and a somewhat remarkable mode of representing what would by analogy be called arithmetical multipliers.

3. A perfect quadratic system, the interpretation of which has considerable resemblance to that of the first-mentioned system, and is completely attainable, though not of great interest.

4. Three perfect cubic systems, each irreconcileable with the others, though closely connected with them. Each system presents a triple trigonometry, the cosine and *two sines* of which are each a function of two angles; but these can be easily expressed as functions of common circular and hyperbolic sines and cosines. The interpretations of these systems are very imperfect, and appear to present great difficulty, but their symbolical character is unimpeachable.

5. A perfect biquadratic system, which is of a redundant character, that is, its fundamental form represents a line drawn in space from a given origin, with a symbol to spare, which may represent the time of drawing it, its density, its tendency to a given position, &c. at pleasure. Many interpretations are attainable, but Mr. De Morgan does not pretend to say that he knows the one which ought to be adopted. It is singular that every attempt to reduce this algebra, by assigning a condition among the subsidiary symbols of its fundamental form, leads to an imperfect algebra. The system first mentioned in this abstract is one such result, and fails in its rules of multiplication, as before mentioned. Another is obtained, which is perfect as to its rules of multiplication, &c., but fails in its rules of addition.

Mr. De Morgan concludes by giving some formulæ which may be useful to those who would try to interpret algebra of three dimensions by the use of solid angles in the place of plane ones.

December 9, 1844.

On the Values of the Sine and Cosine of an Infinite Angle. By the Rev. S. Earnshaw, of St. John's College.

It has been usual with mathematicians to write zero as an equivalent for both $\sin x$ and $\cos x$ when x becomes infinite. The object

of this paper is to examine into the propriety of this usage. The inquiry derives importance from its bearing on the general correctness of Fourier's theorem for the transformation of functions, and from its affecting the truth of many remarkable results in definite integrals. Certain principles also which have been assumed and acted on by Poisson, Fourier, Cauchy and others, in treating of periodic infinite series, are examined, and shown to be untenable: for example, it is shown, that as $1-x$ approaches zero, $1-x+x^2-x^3+\ldots$ ad inf. does not approach $1-1+1-1+\ldots$ ad inf. as its limit; that this last series has not a unique value, and that its value is not $\frac{1}{2}$, as has generally been argued. It is also remarked that every series of the form $a_1 x^\alpha + a_2 x^\beta + \ldots a_n x^\nu + \ldots$ is discontinuous in those terms which are at an infinite distance from the first, unless the coefficients tend to zero as n and ν tend to ∞. The truth of this depends on a circumstance which does not seem to have been remarked before, viz. that however small $1-x$ may be, a value of ν can always be found so large that $(1-x)\,\nu$ may be finite, and therefore x^ν, which is equal to $(1-\overline{1-x})^\nu$, is not equal to 1 in the limit, but to $\text{lim. of } (1-x)^\nu$.

It is lastly proved that $\sin\infty$ and $\cos\infty$ are not equivalent to zero, whether we regard them as the results of integration between limits, or as the limiting cases of more general forms.

February 10, 1845.

On the Connexion between the Sciences of Mechanics and Geometry. By the Rev. H. Goodwin, of Caius College.

This paper contains an attempt to determine the ground of the truth of the elementary propositions of mechanics. The remarkable analogy between mechanics and geometry suggests the thought, that perhaps there may be something more than analogy, that in fact the basis of the two may be the same. The author endeavours to show that this is really the case; the ground of the reasoning is, that force is a *physical* expression of the two ideas of *magnitude* and *direction*, of which a straight line is the *geometrical* expression, and therefore that propositions which are true for one event are true for the other. Hence it is argued, that inasmuch as the giving two sides of a triangle gives the third, so that the third may be considered as the *resultant* of the two already given, so if the two sides represent *forces*, the third will still represent the *resultant* of the two forces already given.

Reasoning of this kind does not, of course, admit of a very demonstrative character *primâ facie*; it is the author's design rather to point out a path to the truth, than to assert that he has cleared away every difficulty.

The subject is further elucidated by the application of the remarkable symbol $e^{\theta\sqrt{-1}}$, a symbol which in geometry serves to indicate the direction in which a line is drawn with respect to a given fixed line; the same symbol is perfectly applicable as a sign of affection for forces, and hence the conclusion is strengthened that the ground of truth in the two sciences is the same.

The reasoning of this paper extends not only to forces, but also to velocities and moments, and to all expressions of whatever kind of the pure ideas of *magnitude* and *direction*.

If the author's reasoning be sound, the elementary propositions of mechanics are *necessary truths* in as strict, in fact, in exactly the same, sense as the elementary propositions of geometry; and to a mind which dwells upon them, the truths of the one science ought to appear in as axiomatic a light as those of the other.

April 14, 1845.

On the Theories of the Internal Friction of Fluids in Motion, and of the Equilibrium and Motion of Elastic Solids. By G. G. Stokes, M.A., Fellow of Pembroke College.

The theory of the equilibrium of fluids depends on the fundamental principle, that the mutual action of two contiguous portions of a fluid is normal to the surface which separates them. This principle is assumed to be true in the common theory of fluid motion. But although the theory of hydrostatics is fully borne out by experiment, there are many instances of fluid motion, the laws of which entirely depend on a certain tangential force called into play by the sliding of one portion of fluid over another, or over the surface of a solid. The object of the first part of this paper is to form the equations of motion of a fluid when account is taken of this tangential force, and consequently the pressure not supposed normal to the surface on which it acts, nor alike in all directions.

Since the pressure in a fluid, or medium of any sort, arises directly from molecular action, being in fact merely a quantity by the introduction of which we may dispense with the more immediate consideration of the molecular forces, and since the molecular forces are sensible at only insensible distances, it follows that the pressure at any point depends only on the state of the fluid in the immediate neighbourhood of that point. Let the system of pressures which exists about any point P of a fluid in motion be decomposed into a normal pressure p, alike in all directions, due to the degree of compression of the fluid about P, and a system S of pressures due to the motion. The author assumes that the pressures belonging to the system S depend only on the relative velocities of the parts of the fluid immediately about P, as expressed by the nine differential coefficients of u, v and w with respect to x, y and z. [The common notation is here employed.] He assumes, further, that the relative velocities due to any arbitrary motion of rotation may be eliminated

without affecting the pressures of the system S. Choosing for the motion of rotation that for which the angular velocities are $\frac{1}{2}\left(\frac{dw}{dy} - \frac{dv}{dz}\right)$ about the axis of x, with similar expressions for the axes of y and z, the residual relative motion depends on only six independent quantities. Considering only this residual relative motion, the author shows that there are always three directions, which he names *axes of extension*, at right angles to one another, such that if they be made the axes of x_1, y_1, z_1, the resolved parts of the relative velocity of the point P′, whose relative co-ordinates are x_1, y_1, z_1, will be $e'x_1$, $e''y_1$, $e'''z_1$ along the three axes of extension respectively, the point P′ being supposed indefinitely near to P. Thus the system of pressures S is made to depend on the three quantities e', e'', e''', which in the case of an incompressible fluid are connected by the equation $e' + e'' + e''' = 0$. Moreover, on account of the symmetry of the motion, the pressures on planes perpendicular to the axes of extension will be normal to those planes. They will here be denoted by p', p'', p'''.

By what precedes, any one of these pressures, as p', will be expressed by $\phi(e', e'', e''')$, the function ϕ being symmetrical with respect to the second and third variables. For reasons stated in the paper itself, the author was led to take, as the form of the function ϕ, $\mathfrak{C}e' + \mathfrak{C}'(e'' + e''')$. The general expressions for the pressures would thus contain two arbitrary constants (or rather functions of the pressure and temperature), which in the case of an incompressible fluid would unite into one. But it is shown by the author, that in all probability $p' = 0$ when $e' = e'' = e'''$; and he accordingly makes this assumption, which reduces the two constants to one, even in the case of a gas. The expression for p' finally adopted is $\frac{2}{3}\mu(e'' = e''' - 2e')$.

The pressures on three planes passing through P being known, the pressure on any other plane passing through that point may be found by the consideration of the motion of an indefinitely small tetrahedron of the fluid. Thus expressions are obtained for the pressures on planes parallel to the co-ordinate planes. These expressions, however, contain quantities which refer to the axes of extension; and it is necessary to transform them into others containing quantities which refer to the axes of co-ordinates. This transformation is easily effected by means of an artifice, and then no difficulty remains in forming the equations of motion. When μ is supposed to be constant, a supposition which it is shown may in many cases be made, the equations thus obtained are those which would be obtained from the common equations by subtracting

$$\mu\left(\frac{d^2u}{dx^2} + \frac{d^2u}{dy^2} + \frac{d^2u}{dz^2}\right) + \frac{\mu}{3}\frac{d}{dx}\left(\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz}\right)$$

from $\frac{dp}{dx}$ in the first, and making similar changes in the other two.

The particular conditions which must be satisfied at the boundaries

of the fluid are then considered, and the general equations applied to a few simple cases.

On considering these equations the author was led to observe, that both Lagrange's and Poisson's proofs of the theorem that $u\,dx + v\,dy + w\,dz$ is always an exact differential when it is so at any instant (the pressure being supposed equal in all directions), would still apply, whereas the theorem is manifestly untrue when the tangential force is taken into account. This led him to perceive that one objection to these proofs is of essential importance. He has given a new proof of the theorem, which however was not necessary to establish it, as it has been proved by M. Cauchy in a manner perfectly satisfactory.

The methods employed in this paper in the case of fluids apply with equal facility to the determination of the equations of equilibrium and motion of homogeneous, uncrystallized, elastic solids, the only difference being that we have to deal with relative velocities in the former case, and with relative displacements in the latter. The only assumption which it is necessary to make, is that the pressures are linear functions of the displacements, or rather relative displacements, the displacements being throughout supposed extremely small. The equations thus arrived at contain two arbitrary constants, and agree with those obtained in a different manner by M. Cauchy. If we suppose a certain relation to hold good between these constants, the equations reduce themselves to Poisson's, which contain but one arbitrary constant.

The equations of fluid motion which would have been arrived at by the method of this paper if the two constants $\mathfrak{C}$, $\mathfrak{C}'$ had been retained, have been already obtained by Poisson in a very different manner. The author has shown, that according to Poisson's own principles, a relation may be obtained between his two constants, which reduces his equations to those finally adopted in this paper.

There is one hypothesis made by Poisson in his theory of elastic solids, by virtue of which his equations contain but one arbitrary constant, which the author has pointed out reasons for regarding as improbable. He has also shown that there is ground to believe that the cubical compressibility of solids, as deduced by means of Poisson's theory from their extensibility when formed into rods or wires, is much too great, a conclusion which he afterwards found had been previously established by the experiments of Prof. Oersted.

The equations of motion of elastic solids with two arbitrary constants, are the same as those which have been obtained by different authors as the equations of motion of the luminiferous æther in vacuum. In the concluding part of his paper the author has endeavoured to show that it is probable, or at least quite conceivable, that the same equations should apply to the motion of a solid, and to those very small motions of a fluid, such as the æther, which according to the undulatory theory constitute light.

May 12, 1845.

On the Aberration of Light. By G. G. Stokes, M.A., Fellow of Pembroke College.

In the common explanation of aberration, it is supposed that light comes in a straight line from a heavenly body to the surface of the earth, except in so far as it is bent by refraction. This, of course, would follow at once from the theory of emissions; but it appears at first sight difficult to reconcile with the theory of undulations, unless we make the startling supposition that the æther passes freely through the earth as the earth moves round the sun. The object of this paper is to show that if we make the following suppositions, that the earth in its motion pushes the æther out of its way, that the æther close to the surface of the earth is at rest relatively to the earth, and that light is propagated through the disturbed æther as we suppose sound to be propagated air in motion, the observed law of aberration will still result, provided the motion of the æther be such that $u\,dx + v\,dy + w\,dz$ is an exact differential, where u, v, w are the resolved parts of the velocity of any particle of the æther along the rectangular axes of x, y, z.

On the Pure Science of Magnitude and Direction. By the Rev. H. Goodwin, Fellow of Caius College, and of the Cambridge Philosophical Society.

This memoir may be considered in some degree supplementary to the preceding one by the same author, "On the Connexion of the Sciences of Mechanics and Geometry." In that memoir it was argued, that if the views there advanced were sound, there must be such a science as that of *pure direction*, or rather a *pure science of magnitude and direction* which should include within itself the sciences of geometry, of kinematics, and of mechanics; in this the attempt is made to establish mathematically the fundamental proposition of such a science.

By making use of De Moivre's formula, the author conceives himself to have established this proposition, that if P represents the magnitude of any cause which varies uniformly and continuously into its exact opposite, *i. e.* into $-$P while its direction varies uniformly from a given direction to the exactly opposite direction; and if θ be the angle which the direction of P makes with a given direction, then P is equivalent to two causes, $P\cos\theta$ in that given direction, and $P\sin\theta$ in the direction perpendicular to it.

The author is aware of the improbability which may appear to exist, that so general a proposition should be susceptible of proof without reference to particular instances, and has therefore endeavoured to obviate some objections, which will be more or less strongly felt, according to the nature of the philosophy of knowledge adopted by the mind which makes them, and which in some cases will probably be invincible.

The memoir concludes with some remarks on the general question

of the transition of a quantity from the + to the − affection, which the author conceives to be illustrative of his general design.

December 8, 1845.

On the Heights of the Aurora Borealis of September 17 and October 12, 1833. By Professor Potter, A.M., of Queen's College.

The data for the calculations are almost entirely taken from the conspectuses of the observations printed and distributed in 1833 by the British Association; and although so long time has elapsed, no calculations of the heights of the phænomena, which are the first steps to be taken in finding the nature of the meteor, have, to the author's knowledge, been hitherto published; the only imperfect discussion being given in the Philosophical Magazine for December 1833.

The observers of the display of September 17, were Mr. J. Phillips, at York; Mr. Clare, Mr. Hadfield and the author, at or near Manchester; Professor Airy, at Cambridge; and the Hon. C. Harris, near Gosport.

The observers of that on October 12, were Professor Sedgwick, at Dent; Mr. W. L. Wharton, near Guisborough; Mr. J. Phillips, at York; Mr. Clare, Mr. Hadfield and the author, at or near Manchester; Dr. Robinson, at Armagh; Professor Airy, at Cambridge; and the Hon. C. Harris, at Heron Court.

The observations of the aurora of September 17 at Cambridge at $8^h\ 25^m$ Greenwich time, taken with those at Manchester at $8^h\ 24^m$, give the height of the lower edge of the arch 56 English miles, and of the upper edge 71 miles.

The observations of another arch, seen from $10^h\ 49^m$ to $11^h\ 19^m$ at York, and from $10^h\ 49\frac{1}{2}^m$ to $11^h\ 4\frac{1}{2}^m$ near Gosport, give the height of the lower edge 389 miles.

The observations on October 12, at $7^h\ 56^m$ at York and at $7^h\ 54^m$ at Cambridge, give the height of the upper edge of an arch 72·2 miles.

The observations at Guisborough at $8^h\ 20^m$, and at Heron Court at $8^h\ 22^m$, give the height of the under edge of the arch seen at that time 70·9 miles, and of the upper edge 85·5 miles.

The observations at Dent at $8^h\ 55^m$, taken with others at Manchester at $8^h\ 54^m$, give the height of the upper edge of that arch 84·97 miles.

The last arch remained stationary about a quarter of an hour, and therefore the observations are the more valuable; but combining an observation at Armagh with those at Manchester, the height comes out only 64·47 miles; and even with the utmost allowable latitude to the deductions from the observations, the height comes out 66·5 miles.

The last arch having been noticed to have risen to a higher alti-

tude at the same places, a calculation with the corresponding data gives the height 65·4 miles.

These last three results are remarkably in accordance with each other, but considerably different from those for other places at nearly the same time; so that probably the method which was used, of obtaining a base line by projecting the places of observation upon an intermediate magnetic meridian, is only approximately correct, from the course of the arch over the earth's surface, rather than geometrical reasons.

Another arch was noted by most of the observers from $10^h\ 34^m$ to $10^h\ 45^m$. The observations at Dent at $10^h\ 40^m$, and Heron Court at $10^h\ 37^m$, give the height of the upper edge 59·4 miles.

An observation made by the author on the extent of the arch, on September 17, upon the horizon at $8^h\ 40\frac{1}{2}^m$, and its altitude, for application to the formula he has given in the Edinburgh Journal of Science, before it was joined with the Philosophical Magazine, for determining the height from observations at one place by the help of an hypothesis, gave the height 53·9 miles, which is a near approximation to the height found by the trigonometrical method for $8^h\ 25^m$.

The author concludes that the meteor occurs immediately beyond the ordinary limits assigned to the earth's atmosphere, and from that to very much greater altitudes, as shown by many other calculations; and states his conviction that the meteor will be some time observed with much more accurate means than hitherto, from its connexion with the earth's magnetism.

February 23, 1846.

Analytical Investigation of the Disease prevalent in the Potato during the year 1845. By Geo. Kemp., M.D., Pet. Coll.

This communication may be resolved into two parts; the analysis of the diseased portion of the potato, as compared with Boussingault's analysis of the healthy tuber, and certain deductions derived from the empirical formulæ proposed as representing their respective compositions.

The author recognises three stages of the disease: the first appearing as dark brown patches under the skin; the second as striæ of the same colour proceeding towards the centre; and the third as a soft, pultaceous, blackish, and offensive mass, in which all traces of organization are lost.

From the impossibility of isolating the portions affected by the disease, in the first two stages, from the surrounding sound parts, the examination was principally directed to the third stage.

A potato having been selected in which the above characters were well-developed, a sufficiently large portion for comparison still remaining perfectly sound, gave the following results as indicative

of the relative proportions of organic and inorganic matter of the sound and unsound part.

Of the sound portion,—

I.	247 milligrammes gave		10·5 milligrammes of ash.		
II.	205·5	...	...	8·5	... ...

Of the unsound portion,—

I.	311 milligrammes gave		18 milligrammes of ash.		
II.	234	...	...	13	... ...
III.	236	...	...	13·5	... ..

Or, reducing to 100 parts, the sound portion consists of—

	I.	II.
Organic matter....	95·75	95·86
Inorganic matter ..	4·25	4·14
	100·00	100·00

whilst the unsound portion gives,—

	I.	II.	III.
Organic matter	94·22	94·45	94·29
Inorganic matter....	5·78	5·55	5·71
	100·00	100·00	100·00

The mean of the former is 4·19 per cent. of ash, that of the latter 5·68, making an excess of 1·49 per cent. arising from loss of organic matter.

The ultimate analysis of the unsound portion afforded the following results:—

I.	155 milligrammes gave carbonic acid			238·5, water 98.		
II.	132	...	... ...	202	...	78.
III.	163	...	... ...	251, water not estimated.		

Mean of two analyses for nitrogen, after the method of MM. Varrentrapp and Will, 1·23 per cent.

These data furnish the following summary:—

	I.	II.	III.
Carbon	42·09	41·73	41·99
Hydrogen ..	7·02	6·56	7·02
Nitrogen....	1·23	1·23	1·23
Oxygen	43·98	44·80	44·08
Ash........	5·68	5·68	5·68
	100·00	100·00	100·00

The analysis of the sound potato, by Boussingault, is as follows:—

Carbon	44·1
Hydrogen	5·8
Nitrogen..........	1·2
Oxygen	43·9
Ash..............	5·0
	100·0

The object of the second part of the communication is to show, that, while Boussingault's analysis of the sound potato may be expressed by an empirical formula, representing the elements of proteine, starch, and cellulose, the analysis of the tuber, after undergoing the action of the late prevalent disease, admits of no such solution; but may be expressed by an empirical formula, representing proteine, starch, and butyric acid, with a very large excess of the elements of water.

Butyric acid has been found in the diseased potato by Mr. Tilley; but the author's principal object is to connect the changes developed by his analyses with the researches of Erdmann, Marchand, and Scharling, on the germination of seeds and tubers. These researches are totally independent and irrespective of the disease in question, whilst it is clear that the same changes occur in both cases. After reviewing the physical circumstances with respect to soil and culture, which have proved remarkably favourable to the development of the morbid changes, the author arrives at the general conclusion, that the disease in question essentially consists in an unnatural tendency to premature germination.

PROCEEDINGS

OF THE

CAMBRIDGE PHILOSOPHICAL SOCIETY.

February 1846.

The Mathematical Theory of the two great Solitary Waves of the first Order. By S. Earnshaw, M.A.

The nomenclature of this paper is adopted from a Report on Waves by Mr. J. S. Russell, printed in the Proceedings of the British Association. From the extreme comprehensiveness of the equations of fluid motion, the author infers a necessity of appealing to experiments for the suggestion of data which may be used in modifying the generality of those equations so as to suit actual cases of known fluid motion. With this view he has made use of the experiments recorded in Mr. Russell's report, and thence selected the two following properties:—1st. The velocity of transmission of a wave in a uniform canal is constant. 2nd. The horizontal velocity is the same for all particles situated in a vertical plane, cutting the axis of the canal at right angles. By reference to Mr. Russell's report, it will be seen that these two properties, selected on account of their simplicity and ready experimental examination, are distinguishing characteristics of what he has denominated the two great solitary waves of the first order. By the aid of them the equations of motion take such modified forms as to admit of exact integration; so that without employing any analytical approximations the author is enabled to obtain theoretical expressions for all the circumstances of the two solitary waves. The results are tested by a comparison of the velocities of transmission of various waves given by theory and by experiment. The greatest difference of these in the case of the positive wave is not found to exceed $\frac{1}{35}$th part of the whole velocity; but in the case of the negative wave it is found to be much greater, and to amount in one instance to as much as $\frac{1}{8}$th of the whole velocity. The reason of this discrepancy is conjectured; and the agreement in the case of the positive wave is considered to be exact.

It is found in the course of the investigation that one of the necessary conditions of fluid motion is not satisfied; and it is shown that it cannot be satisfied as long as the two principles, adopted from Mr. Russell's report, are supposed to coexist. They are proved in fact to be incompatible with each other. But as the second prin-

ciple was found by Mr. Russell to be so nearly exact that he could not detect any deviation from it in his experiments, it is shown by theory that from this circumstance there will be a rapid degradation of the summit of the wave, and a consequent loss of the velocity of its transmission, both which results of theory were observed to be true experimentally. The memoir concludes with pointing out the agreement of theory with some minor phænomena noticed by Russell.

May 11, 1846.

A theory of Luminous Rays on the Hypothesis of Undulations. By the Rev. J. Challis, M.A., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge.

In this communication, the æther, which is supposed to be the medium of the transmission of light, is regarded as a continuous fluid substance, such that small increments of its pressure are proportional to small increments of density, and is treated mathematically according to hydrodynamical principles. The author shows, by means of the usual hydrodynamical equations, and by an additional equation of continuity, the existence and necessity of which he has considered in the Cambridge Philosophical Transactions (vol. vii. part iii. pp. 385 and 386), that a given slender cylindrical portion of the fluid may continue in motion without tendency to lateral spreading, while all other parts remain at rest. It is shown,—1, that the motion in this filament of fluid may be propagated with a uniform velocity; 2, that in one straight line, which may be called its axis, the motion is entirely longitudinal; 3, that at all other points the motion is partly longitudinal and partly transversal; 4, that the motion is vibratory, the vibrations both longitudinal and transversal following the law of sines; 5, that the condensation (s) in any transverse plane, at a point whose co-ordinates in that plane reckoned from the axis are x and y, is given by the equation

$$\frac{d^2s}{dx^2}+\frac{d^2s}{dy^2}+gs=0,$$

g being a certain constant. It follows that the condensation in any transverse plane, being determined by a partial differential equation, is arbitrary, and by consequence that the transverse velocity varies at a given time from point to point of any transverse plane in an arbitrary manner. To obtain the foregoing equation, it is assumed that the condensation at any point of a transverse plane, has to the condensation at the intersection of the plane with the axis, a ratio not variable with the time.

Each fluid filament in vibration is supposed in this theory to correspond to a *ray* of light. The vibrations in different fluid filaments may co-exist, and consequently rays be propagated in the same direc-

tion independently of each other. A ray of common light has the condensation symmetrically arranged about the axis.

May 25, 1846.

A Theory of the Polarization of Light on the Hypothesis of Undulations. By the same Author.

This paper is a continuation of the foregoing. A ray in which the condensation is not arranged symmetrically about the axis is considered to be *polarized*. Polarization in this theory corresponds to difference of condensation in different directions transverse to the axis of the ray. The sensation of light is due to the *transverse* vibrations. By assuming that the bifurcation of a ray takes place so that the transverse velocity at each point is resolved into two velocities at right angles to each other, and that these are respectively the velocities at the corresponding points of the two parts into which the ray is divided, Professor Challis finds,—1, that if the original ray be one of common light, the two parts are symmetrical about planes at right angles to each other passing through the axis, and are each of half the intensity of the original ray; 2, that if the original ray has been once polarized, the ratio of the two parts is equal to the square of the tangent of the angle which the plane of the second polarization makes with that of the first; 3, that whether the original ray be one of common light or a polarized ray, the two parts, on pursuing the same path, form a compound ray the intensity of which is independent of the difference of phase. According to this theory, elliptically or circularly polarized light is produced whenever a ray of first polarization is divided into two parts which subsequently pursue the same path in different phases. If the parts be made to meet in the same phase, they constitute the original polarized ray. Hence is explained the necessity of the analysing plate for the production of colours by polarized rays transmitted through thin pieces of uniaxal or biaxal crystals. The compound rays, if received directly by the eye on leaving the crystal, would be of the same intensity whatever be the difference of phase. But when they fall on the plate, those incident in the same phase, being equivalent to rays of first polarization, are incapable of reflexion, while the remainder, which are incident in the form of elliptically or circularly polarized light, are reflected in different degrees of intensity according to the difference of phase. The author states that he has extended this theory to the phænomena of double refraction.

On a Change in the State of Vision of an Eye affected with a mal-formation. By G. B. Airy, Esq., Astronomer Royal.

Twenty years ago, the author communicated to the Society a statement of the effects of a mal-formation in his left eye. The rays of light coming from a luminous point, and falling on the whole surface

of the pupil, do not converge to a point at any position within the eye, but converge so as to pass through two lines at right angles to each other, and, in the ordinary position of the head, inclined to the vertical, as formerly described (Transactions of the Society, vol. ii.). As the luminous point is moved further from or nearer to the eye, the image of the point becomes a straight line in one or other of the positions above-mentioned. Since 1825 the inclinations of the two focal lines to the vertical, their length, and their sharpness do not appear to have undergone any sensible change, but the distances at which the luminous point must be placed to bring the focal lines respectively exactly upon the retina are increased, having been formerly 3·5 and 6 inches, and being now 4·7 and 8·9 inches. Thus while the shortsightedness of the eye is diminished the astigmatism remains the same.

On the Geometrical Representation of the Roots of Algebraic Equations. By the Rev. H. Goodwin, late Fellow of Caius College, and Fellow of the Cambridge Philosophical Society.

The changes of value of any function of x, $f(x)$, may be very clearly, and for some purposes very usefully represented, by tracing the curve defined by the equation $z=f(x)$; and the positive and negative roots of the equation $f(x)=0$ will be the distances from the origin at which the curve cuts the axis of x.

In this memoir a similar method is applied to the representation of the changes of value of a function of (x), corresponding not only to real values of x, but also to values of the form $x+y\sqrt{-1}$. If we make $z=f(x+y\sqrt{-1})$, and restrict ourselves to real values of z, the equation separates itself into two, which, it is shown, may be represented symbolically by

$$z=\cos\left(y\frac{d}{dx}\right)f(x)$$

$$\text{and } 0=\sin\left(y\frac{d}{dx}\right)f(x),$$

and these will correspond to a curve of double curvature, the intersections of which with the plane of xy will determine by the distance of those points from the origin the imaginary roots of the equation $f(x)=0$.

The properties of this curve are fully discussed for the case of $f(x)$ being equivalent to $x^n+p_1\,x_{n-1}+p_2\,x_{n-2}+\,\ldots\,+p_n$, where $p_1\,p_2$ $\ldots\,p_n$ are real; and the following results are obtained.

1. The ordinate of the curve admits of no maximum or minimum value.

2. The curve goes off into infinite branches, which lie in asymptotic planes equally inclined to each other, and which tend alternately to positive and negative infinity.

3. Any plane parallel to the plane of xy cuts the curve in n points and no more.

From this last result the existence of n roots and no more for an equation of n dimensions is the immediate result.

Several well-known theorems are deduced from this view of the subject, and are given as illustrations.

The actual curves are traced, corresponding to the various cases of the quadratic, the cubic, and the biquadratic equations, and to the equation $x^n-1=0$.

In the conclusion of the memoir it is remarked that the results obtained are not exclusively applicable to the case of algebraic equations, and the methods are applied to the case of $f(x)=\sin x$.

The author trusts that the contents of this memoir, though not adding to the number of known theorems, may yet be useful as putting the subject in a new light, and as furnishing a method of demonstrating the existence of the roots of algebraic equations more simple and direct than any other which he has seen.

Cases of Morbid Rhythmical Movements, with observations. By G. E. Paget, M.D., Fellow of Caius College and of the Royal College of Physicians, London.

Seven cases were related. The movements were vibratory, rotatory, bowing, &c. In some of the cases they were incessant; in others paroxysmal; and in others again they were of both kinds, the predominant movement being replaced at intervals by distinct paroxysms.

On a comparison of these cases with the few others on record, and with the experiments of Flourens and Majendie, it was inferred as probable, that one class of the movements, viz. the rotatory, depended on disorder in the cerebellum or its transverse commissure, the pons. With regard to the other movements, it appeared that there were no sufficient grounds for even a probable conjecture as to the particular part of the encephalon, the excitement or disorder of which might act as an immediate cause of the movements.

The *remote* causes were such as under other circumstances are known to excite the common convulsive diseases, such as chorea anp epilepsy. These remote causes were in most cases *eccentric*.

November 9, 1846.

On the Structure of the Syllogism, and on the application of the Theory of Probabilities to Questions of Argument and Authority. By Professor De Morgan.

The object of this paper is twofold: first, to establish two distinct theories of the syllogism, both differing materially from that of Aristotle, and each furnishing a general canon for the detection of all its legitimate forms of inference; secondly, to investigate the mode in which the distinctive character of the two great sources of conviction, *argument* and *authority*, affects the application of the notion of probability to questions not admitting of absolute demonstration.

The two theories of the syllogism arise out of simple notions connected with the *forms* of propositions and their *quantities*. The difference between a positive and negative assertion is not essential, but depends on the manner in which objects of thought are described by language. If Y and *y* be names so connected that each contains everything which is not in the other, and the two have nothing in common (a relation which is described by calling them *contrary* names), the propositions 'Every X is Y' and 'no X is *y*' are simply identical. In the same manner, the particular and universal proposition are only accidentally distinct. If in 'some Xs are Ys' the Xs there specified had had a name belonging to them only, say Z, then the preceding proposition would have been identical in meaning with 'every Z is Y.'

From the above it is made to follow, that every legitimate syllogism can be reduced to one of universal affirmative premises, either by introduction of contrary terms, or invention of subgeneric names.

In considering the nature of the simple proposition, Mr. De Morgan uses a notation proposed by himself. Thus—

Every X is Y is denoted by	X)Y	A
No X is Y	X.Y	E
Some Xs are Ys .. .	XY	I
Some Xs are not Ys ..	X:Y	O

and names which are contraries are denoted by large and small letters. Aristotle having excluded the contrary of a name from formal logic, and having thereby reduced the forms of proposition to four, these forms (*universal affirmative, universal negative, particular affirmative, particular negative*) the writers on logic in the middle ages represented by the letters A, E, I, O. Thus X)Y and Y)X are equally represented by A. When contraries are expressly introduced, all the forms of assertion or denial which can obtain between two terms and their contraries, are *eight* in number; and the most convenient mode of representing them is as follows:—Let the letters A, E, I, O have the above meaning, but only when the order of subject and predicate is XY. Then let *a, e, i, o* stand for the same propositions, after *x* and *y*, the contraries, are written for X and Y. The complete system then is—

A=X)Y	*a*=*x*)*y*=Y)X
O=X:Y	*o*=*x*:*y*=Y:X
E=X.Y	*e*=*x*.*y*
I=XY	*i*=*xy*

and every form in which subject and predicate are in any manner chosen out of the four X, Y, *x*, *y*, so that one shall be either X or *x*, and the other either Y or *y*, is reducible to one or other of the preceding.

The propositions *e* and *i*, which are thus newly introduced, are only expressible as follows, with reference to X and Y.

(*i.*) *There are things which are neither* X *nor* Y.

(*e.*) *There is nothing but is either* X *or* Y *or both.*

The connexion of these eight forms is fully considered, and the

various syllogisms to which they lead. Rejecting every form of syllogism in which as strong a conclusion can be deduced from a weaker premise; rejecting, for instance,

$$Y)X+Y)Z=XZ$$

because XZ equally follows from Y)X+YZ, in which YZ is *weaker* than Y)Z—all the forms of inference are reduced to three sets.

1. A set of two, called *single* because the interchange of the terms of the conclusion does not alter the syllogism. Neither of these forms are in the Aristotelian list. One of them is

$$X)Y+Z)Y=xz;$$

or *if every* X *be a* Y, *and also every* Z, *then there are things which are neither* X *nor* Z; *namely, all which are not* Ys.

2. A set of six, in which the interchange produces really different syllogisms of the same form, and in which both premises and conclusion can be expressed in terms of *three names*, without the contrary of either. This set includes the whole Aristotelian list, except those in which a weaker premise will give as strong a conclusion, or the one in which the same premises will give a stronger conclusion.

3. A set of six resembling the last in everything but this, that no one of them is expressible without the new forms *e* and *i*; that is, requiring three names and the contraries of one or more of them.

Those of the third set are not reducible to Aristotelian syllogisms, as long as the eight standard forms of assertion are adhered to.

The second theory of the syllogism has its principles laid down in the memoir before us; but those principles are only applied to the evolution of the cases which are not admitted into the Aristotelian system. The formal statement of the manner in which the ordinary cases of syllogism are connected with those peculiar to this second system is contained in an *Addition*.

In providing that premises shall certainly furnish a conclusion, the common system requires that one at least of the premises shall speak *universally* of the middle term; that is, shall make its assertion or denial of *every* object of thought which is named by the middle term. Mr. De Morgan points out that this is not necessary: m being the fraction of all the cases of the middle term mentioned in one premise, and n in the other, all that is necessary is that $m+n$ should be greater than unity. In such case, the real middle term, being the collection of all the cases by comparison of which with other things inference arises, is the fraction $m+n-1$ of all the possible cases of the middle term. Thus, from the premises 'most Ys are Xs' and 'most Ys are Zs,' it can be inferred that some Xs are Zs, since m and n are both greater than one-half. The assignment of definite quantity to the middle term in both premises, gives a canon of inference, of which the Aristotelian rule is only a particular case.

In the addition above alluded to, this same canon, namely 'that more Ys in number than there exist separate Ys shall be spoken of in both premises together,' is made to take the following form:—If in an affirmation or negation, in 'As are Bs' and 'As are not Bs,' definite numerical quantity be given to both subject and predicate, if

it be stated how many As are spoken of and how many Bs—the number of *effective* cases of the middle term is seen to be the number of *subjects* in an affirmative proposition, whether the middle term be subject or predicate. Hence, defining the effective number of a premise to be the number of subjects if the proposition be affirmative, and the number of cases of the middle term if it be negative, all that is necessary for inference (over and above the usual condition that both premises must not be negative) is that the sum of the effective numbers of the two premises shall exceed the number of existing cases of the middle term; and the excess (being the fraction denoted by $m+n-1$ in the Memoir) gives the number of cases in which inference can be made.

To attempt to combine these two systems of *form* and of *quantity* is rendered useless by language not possessing the forms of mixed assertion and denial, which the syllogisms deduced from the combination would require. As far as the combination can, in Mr. De Morgan's opinion, be made, nothing is required but a distinct conception of, and nomenclature for, the usual modes of expressing a logical form, and implying one or the other of the alternations which the mere expression leaves unsettled. Mr. De Morgan proposes the following language.

Two names are *identical* when each contains all that the other contains: but when all the first (and more) is contained in the second, then the first is called a *subidentical* of the second, and the second a *superidentical* of the first. Two names are contrary when everything (or everything intended to be spoken of) is in one or the other and nothing in both. But when the two names have nothing in common, and do not between them contain everything, they are called *subcontraries* of one another. And again, if everything be in one or the other, and some things in both, they are called *supercontraries* of one another. Lastly, if the two names have each something in common and something not in common, and moreover do not between them contain everything, each is called a *complete particular* of the other. A table is then given, which contains every form of complex syllogism.

If X and Z be the terms of the conclusion, and both be described in terms of Y, the middle term: it can be seen from this table what can be affirmed and what denied, of X with respect to Z. For instance, if X be supercontrary of Y, and Z subcontrary, then X must be a superidentical of Z: but if X and Z be both subidenticals of Y, nothing can be affirmed; only it may be denied that X is either contrary or supercontrary of Z.

The remaining part of this paper relates to the application of the theory of probabilities above-mentioned. Mr. De Morgan asserts that no conclusion of a definite amount of probability can be formed from argument alone; but that all the results of argument must be modified by the testimony to the conclusion which exists in the mind, whether derived from the authority of others, or from the previous state of the mind itself. The foundation of this assertion is the circumstance that the insufficiency of the argument is no index of

the falsehood of the conclusion. Various cases are examined; but it must here be sufficient to cite one or two results.

If μ be the probability which the mind attaches to a certain conclusion, a the probability that a certain argument is valid, and b the probability that a certain argument for the contradiction is valid: then the probability of the truth of the conclusion is

$$\frac{(1-b)\mu}{(1-b)\mu+(1-a)(1-\mu)}.$$

If $b=0$, or if there be no argument against, and if the mind be unbiassed, or if $\mu=\frac{1}{2}$, this becomes

$$\frac{1}{2-a} \text{ or } a+\frac{(1-a)^2}{2-a}.$$

For this writers on logic generally substitute a, confounding the absolute truth of the conclusion with the validity of the argument, and neglecting the possible case of the argument being invalid, and yet the conclusion true.

November 23, 1846.

On a New Notation for expressing various Conditions and Equations in Geometry, Mechanics and Astronomy. By the Rev. M. O'Brien.

If A, P, P′ be any three points in space, whether in the same straight line or not, and if the lines AP and AP′ be represented in magnitude and direction by the symbols u and u', then, according to principles now well-known and universally admitted, the line PP′ is represented in magnitude and direction by the symbol $u'-u$. Now if AP and AP′ be equal in magnitude, and make an indefinitely small angle with each other, PP′ is an indefinitely small line at right angles to AP, and $u'-u$ becomes du. Hence it follows, that, if u be the symbol of a line of invariable magnitude, du is the symbol of an indefinitely small line at right angles to it; and therefore, if λ be any arbitrary coefficient, λdu is the general expression for a right line perpendicular to u.

The sign λd therefore indicates perpendicularity, when put before the symbol of a line of invariable length. The object of the author is to develope this idea, and to show that it not only leads to a simple method of expressing perpendicularity, but also furnishes a notation of considerable use in expressing various conditions and equations in geometry, mechanics, astronomy, and other sciences involving the consideration of *direction* and *magnitude*.

The author first reduces the sign λd to a more convenient form, which not only secures the condition that u is invariable in length, but also defines the magnitude and direction of the perpendicular which λdu denotes. This he does in the following manner. He assumes

$$u=x\alpha+y\beta+z\gamma,$$

(where $\alpha\ \beta\ \gamma$ represent three lines, each a unit in length, drawn at right angles to each other, and $x\ y\ z$ are any arbitrary numerical coefficients,) and supposes that the differentiation denoted by d affects $\alpha\ \beta\ \gamma$, but not $x\ y\ z$. This secures the condition that u is invariable in length, and leads to the following expression for λdu, viz.

$$\lambda du=(zy'-z'y)\alpha+(xz'-x'z)\beta+(yx'-y'x)\gamma,$$

$x'\ y'\ z'$ being arbitrary coefficients.

Assuming $u'=x'\alpha+y'\beta+z'\gamma$, it appears from this expression for λdu, that $du=0$ when $u=u'$, and therefore that d denotes a differential taken on the supposition that u' is constant.

On this account the author substitutes the symbol $D_{u'}$ in place of λd; he then shows that the operation $D_{u'}$ is *distributive* with respect to u' (*i. e.* that $D_{u'+u''}=D_{u'}+D_{u''}$), and to indicate this he elevates the subscript index u', and writes $Du'.u$ instead of $D_{u'}u$. Thus he obtains the expression

$$Du'.u=(zy'-z'y)\alpha+(xz'-x'z)\beta+(yx'-y'x)\gamma.$$

From this it follows that $Du'.u$ is a line perpendicular both to u' and u, and that the numerical magnitude of $Du'.u$ is $rr'\sin\theta$, where r and r' are the numerical magnitudes of u and u', and θ the angle made by u and u'.

Having investigated the principal properties of the operation $Du'.$, the author, by a similar method, obtains another notation, $\Delta u'.u$, which represents the expression $xx'+yy'+zz'$, or $rr'\cos\theta$. He then gives some instances of the application of these two notations to mechanics, which may be briefly stated as follows:—

1st. If U, U', U'', &c. be the symbols* of any forces acting upon a rigid body, and u, u', u'', &c. the symbols† of their respective points of application, then the six equations of equilibrium are included in the two equations

$$\Sigma U=0 \text{ and } \Sigma Du.U=0.$$

2nd. That these two equations are the necessary and sufficient conditions of equilibrium, may be proved very simply from first principles by the use of the notation Du.

3rd. The theory of couples is included in the equation $\Sigma Du.U=0$. In fact the symbol $Du.U$ expresses, in magnitude and direction, the axis of the couple by which the force U is transferred from its point of application U to the origin.

4th. Supposing that the forces U, U', U'', &c. do not balance each other, and putting $\Sigma U=V$, $\Sigma Du.U=W$, we may show immediately, by the use of the notation Δu, that the condition of there being a single resultant is

$$\Delta V.W=0;$$

and when there is not a single resultant, the axis of the couple of minimum moment is

* By the symbol of a force is meant the expression $X\alpha+Y\beta+Z\gamma$, where X Y Z are the three components of the force.

† By the symbol of a point is meant the expression $x\alpha+y\beta+z\gamma$, where $x\ y\ z$ are the coordinates of the point.

$$\frac{\Delta V.W}{\Delta V.V.}.V.$$

5th. The three equations of motion of a rigid body about its centre of gravity are included in the equation

$$\frac{d}{dt}\left(\Sigma D\varkappa.\frac{d\varkappa}{dt}\delta m\right)=\Sigma D\varkappa.U\delta m\,; \quad . \quad . \quad . \quad . \quad . \qquad (1.)$$

$\varkappa$ being the symbol of the position of any particle δm of the body, and U the symbol of the accelerating force acting on δm.

6th. If ω be assumed to represent the expression $\omega_1\alpha+\omega_2\beta+\omega_3\gamma$, where ω_1, ω_2, ω_3 are the angular velocities of the planes of yz, zx, xy about the axes of x, y, z respectively, then the symbol of the velocity of δm is $D\omega.\varkappa$; from which follow immediately the three well-known equations,

$$\frac{dx}{dt}=\omega_2 z-\omega_3 y, \quad \frac{dy}{dt}=\omega_3 x-\omega_1 z, \quad \frac{dz}{dt}=\omega_1 y-\omega_2 x.$$

The symbol ω represents in direction the axis of instantaneous rotation, and in magnitude the angular velocity about that axis.

7th. The equation (1.) may be reduced to the form

$$\frac{d.}{dt}\{A\omega_1\alpha+B\omega_2\beta+C\omega_3\gamma\}=\Sigma D\varkappa.U\delta m,$$

which includes Euler's three equations of motion about a fixed point.

8th. If the forces U, U′, U″, &c. arise from the attraction of a distant body, the symbol of whose position is $\varkappa'$, this equation may be further reduced to the form

$$\frac{d}{dt}\left(A\omega_1\alpha+B\omega_2\beta+C\omega_3\gamma\right)=\frac{3m'}{r'^5}D\varkappa'.(Ax'\alpha+By'\beta+Cz'\gamma).$$

9th. In the case of the earth attracted by the sun or moon, this equation becomes

$$\frac{d\omega}{dt}=\frac{3m'}{r'^5}\lambda(\Delta\varkappa'.\gamma)(D\varkappa'.\gamma)\,;$$

γ being the polar axis, and $\lambda=\dfrac{C-A}{A}$.

10th. The mean daily motion of γ is given by the equation

$$\frac{d\gamma}{dt}=\frac{3m'}{nr'^5}\lambda(\Delta\varkappa'.\gamma)(D\varkappa'.\gamma)\,;$$

which equation gives immediately all the well-known expressions for solar and lunar precession and nutation, for $\frac{d\gamma}{dt}$ is the symbol of the velocity of the north pole, representing that velocity both in magnitude and direction.

Supplement to a Memoir on some cases of Fluid Motion. By G. G. Stokes, M.A., Fellow of Pembroke College, Cambridge.

In a former paper the author had given the mathematical calcula-

tion of an instance of fluid motion, which seemed to offer an accurate means of comparing theory and observation in a class of motions, in which, so far as the author is aware, they had not been hitherto compared. The instance referred to is that in which a vessel or box of the form of a rectangular parallelepiped is filled with fluid, closed, and made to perform small oscillations. It appears from theory that the effect of the inertia of the fluid is the same as that of a solid having the same mass, centre of gravity and principal axes, as the solidified fluid, but different principal moments of inertia. In this supplement the author gave a series for the calculation of the principal moments, which is more rapidly convergent than one which he had previously given. It is remarkable that these series, though numerically equal, appear under very different forms, the nth term of the latter containing exponentials of the forms $\varepsilon^{n\pi r}$ and $\varepsilon^{\frac{n\pi}{r}}$, while the nth term of the former contains exponentials of the second form only. In conclusion, the author referred to some experiments which he had performed with a box, such as that described, filled with water, employing the method of bifilar oscillations. The moment of inertia of the fluid about an axis passing through its centre of gravity (*i. e.* the moment of inertia of the imaginary solid which may be substituted for the fluid), was a little greater as determined by experiment than as determined by theory, as might have been expected, since the friction of the fluid was not considered in the calculation. The difference between theory and experiment varied in different cases from the $\frac{1}{13}$th to the $\frac{1}{21}$st part of the whole quantity.

December 7, 1846.

On the Principle of Continuity in reference to certain results of Analysis. By Professor Young of Belfast College.

The object of this paper is to inquire into the influence of the law of continuity, as it affects the extreme or ultimate values of variable functions, more especially those involving infinite series and definite integrals.

The author considers that this influence has hitherto been improperly overlooked; and that to this circumstance is to be attributed the errors and perplexities with which the different theories of those functions are found to be embarrassed. He shows that every particular case of a general analytical form—even the ultimate or limiting case—must come under the control of the law implied in that form; this law being equally efficient throughout the entire range of individual values. Except in the limiting cases, the law in question is palpably impressed on the several particular forms; but at the limits it has been suffered to escape recognition, because indications of its presence have not been actually preserved in the notation.

It is in this way that the series $1-1+1-1+$ &c. has been confounded with the limits of the series $1-x+x^2-x^3+$ &c.; these

limits being arrived at by the continuous variation of x from some inferior value up to $x=1$, and from some superior value down to $x=1$. It is shown however that the series $1-1+$ &c. has no equivalent among the individual cases of $1-x+x^2-$ &c., with which latter, indeed, it has no connexion whatever.

By properly distinguishing between the real limits, and what is generally confounded with them, the author arrives at several conclusions respecting the limiting values of infinite series directly opposed to those of Cauchy, Poisson, and others. And to prevent a recurrence of errors arising from a neglect of the distinction here noticed, he proposes to call such an isolated series as $1-1+1-$&c. *independent* or *neutral*; and the extreme cases of $1-x+x^2-$&c., *dependent* series: the difference between a dependent and a neutral series becomes sufficiently marked, as respects notation, by introducing into the former what the author calls the *symbol of continuity*, which indeed is no other than the factor, whose ascending powers Poisson introduces—and, as here shown, unwarrantably—into the successive terms of strictly *neutral* series; thus bringing such series under the control of a law to which in reality they owe no obedience.

An error somewhat analogous to this is shown to be committed in the treatment of certain definite integrals, which are here submitted to examination and correction, and some disputed and hitherto unsettled points in their theory fully considered. The author is thus led to what he considers an interesting fact in analysis; viz. that the *differentials* of certain forms require *indeterminate corrections*, in a manner similar to that by which *determinate* corrections are introduced into *integrals*; and he attributes to the neglect of these the many erroneous summations assigned to certain trigonometrical series. This is illustrated by a reference to the processes of Poisson.

The paper concludes with some observations on what has been called *discontinuity*; a term which the author thinks is sometimes injudiciously employed in analysis, and prefers to treat discontinuous functions as implying distinct continuities; and by considering these in accordance with the principles established in the former part of the paper, he arrives at results for definite integrals of the form $\int_{-\infty}^{+\infty} x^{-p}\,dx$ totally different from those obtained by Poisson. Two notes are appended to the paper; one explaining what the author denominates *insensible convergency* and *insensible divergency*, and the other discussing some conclusions of Abel in reference to certain trigonometrical developments.

March 1, 1847.

On the Theory of Oscillatory Waves. By G. G. Stokes, M.A., Fellow of Pembroke College.

The waves which form the subject of this paper are characterized

by the property of being propagated with a constant velocity, and without degradation, or change of form of any kind. The principal object of the paper is to investigate the form of these waves, and their velocity of propagation, to a second approximation; the height of the waves being supposed small, but finite. It is shown that the elevated and depressed portions of the fluid are not similar, as is the case to a first approximation; but the hollows are broad and shallow, the elevations comparatively narrow and high. The velocity of propagation is the same as to a first approximation, and is therefore independent of the height of the waves. It is remarkable that the forward motion of the particles near the surface is not exactly compensated by their backward motion, as is the case to a first approximation; so that the fluid near the surface, in addition to its motion of oscillation, is flowing with a small velocity in the direction in which the waves are propagated; and this velocity admits of expression in terms of the length and height of the waves. The knowledge of this circumstance may be of some use in leading to a more correct estimate of the allowance to be made for leeway in the case of a ship at sea. The author has proceeded to a third approximation in the case in which the depth of the fluid is very great, and finds that the velocity of propagation is increased by a small quantity, which bears to the whole a ratio depending on the square of the ratio of the height of the waves to their length.

In the concluding part of the paper is given the velocity of propagation of a series of waves propagated along the common surface of two fluids, of which the upper is bounded by a horizontal rigid plane. There is also given the velocity of propagation of the above series, as well as that of the series propagated along the upper surface of the upper fluid, in the case in which the upper surface is free. In these investigations the squares of small quantities are omitted.

March 15, 1847.

Contributions towards a System of Symbolical Geometry and Mechanics. By the Rev. M. O'Brien.

The distinction which has been made by an eminent authority in mathematics between *arithmetical* and *symbolical* algebra, may be extended to most of the sciences which call in the aid of algebra. Thus we may distinguish between *symbolical geometry* and *arithmetical geometry*, *symbolical mechanics* and *arithmetical mechanics*. This distinction does not imply that in one division numbers only are used, and in the other symbols, for symbols are equally used in both; but it relates to the degree of generality of the symbolization. In the arithmetical science, the symbols have a purely numerical signification; but in the symbolical they represent, not only abstract quantity, but also all the circumstances which, as it is expressed, *affect* quantity. The arithmetical science is in fact the first step of generalization, the symbolical is the complete generalization.

In this view of the case, the author has entitled his paper Contributions towards a System of Symbolical Geometry and Mechanics. The proposed geometrical system consists, first, in representing curves and surfaces, not by equations, as in the Cartesian method, but by *single symbols*; and secondly, in using the *differential notation* proposed in a former paper* to denote *perpendicularity*, and to express various equations and conditions. The proposed mechanical system is analogous in many respects. Examples of it have already been given in the paper just quoted.

The author uses the term *direction unit* to denote a line of a unity of length drawn in any particular direction; and he employs the symbols $\alpha\ \beta\ \gamma$ to denote any three direction units at right angles to each other.

He defines the position of any point P in space by the symbol representing the line OP (O being the origin) in magnitude and direction. If $x\ y\ z$ be the numerical values of the coordinates of P, and $\alpha\ \beta\ \gamma$ the direction units of the coordinate axes, the expression

$$x\alpha+y\beta+z\gamma$$

represents the line OP in magnitude and direction, and therefore defines the position of P. This expression he calls the *symbol* of the point P.

If r be the numerical magnitude, and s the direction unit of OP, we have

$$rs=x\alpha+y\beta+z\gamma:$$

$r\,s$ is therefore another form for the symbol of the point P.

The following is the method by which the author represents curves and surfaces.

If the symbol of a point involves an arbitrary quantity, or, as it is called, a variable parameter, the position of the point becomes indeterminate, but so far restricted that it will be always found on some line or curve. Hence the symbol of a point becomes the symbol of a line or curve when it involves a variable parameter.

In like manner, when the symbol of a point involves *two* variable parameters, it becomes the symbol of a surface.

The parameters here spoken of are supposed to be numerical quantities. An arbitrary direction unit is clearly equivalent to two such parameters; and therefore, when the symbol of a point involves an arbitrary direction unit, it becomes the symbol of a surface.

The following are examples of this method :—

1. If u be the symbol of any particular point of a right line whose direction unit is s, then the symbol of that right line is

$$u+rs,$$

r being arbitrary.

2. If u be the symbol of the centre of a sphere, and r its radius, the symbol of the surface of a sphere is

$$u+rs,$$

s being an arbitrary direction unit.

* Read Nov. 23, 1846.

3. If u be the symbol of any particular point of a plane, s and s' the direction units of any two lines in the plane, the symbol of the plane is

$$u+rs+r's',$$

r and r' being arbitrary.

4. If s be the direction unit and r the numerical magnitude of the perpendicular from the origin on a plane, the symbol of the plane is

$$rs+\mathrm{D}v.s,$$

v being an arbitrary line symbol, *i. e.* denoting in magnitude and direction any arbitrary line.

5. If u and u' be the symbols of two points, the symbol of the right line drawn through them is

$$u+m(u'-u),$$

m being arbitrary.

6. If u be the symbol of any curve in space, the symbol of the tangent at the point u is

$$u+mdu,$$

m being arbitrary.

7. The symbol of the osculating plane at the point u is

$$u+mdu+m'd^2u,$$

m and m' being arbitrary.

8. If s denotes the length of the arc of the curve, and s the direction unit of the tangent, then

$$s=\frac{du}{ds}.$$

9. $\dfrac{ds}{ds}$ or $\dfrac{1}{ds}\,d\left(\dfrac{du}{ds}\right)$ represents a line equal to the reciprocal of the radius of curvature drawn from the point u towards the centre of curvature, *i. e.* it represents what may be called the *index of curvature* in magnitude and direction.

Hence, since $u=x\alpha+y\beta+z\gamma$, the numerical magnitude of $\dfrac{1}{ds}\,d\left(\dfrac{du}{ds}\right)$ is

$$\frac{1}{ds}\sqrt{\left\{\left(d\frac{dx}{ds}\right)^2+\left(d\frac{dy}{ds}\right)^2+\left(d\frac{dz}{ds}\right)^2\right\}},$$

which is the general expression for the reciprocal of the radius of curvature.

10. The symbol of the normal which lies in the osculating plane is

$$u+md\left(\frac{du}{ds}\right),$$

m being arbitrary.

11. The symbol of *any* normal at the point u, *i. e.* the symbol of the normal plane, is

$$u+\mathrm{D}v.du,$$

v being an arbitrary line symbol.

12. The symbol of the normal perpendicular to the osculating plane is

$$u + m\mathrm{D}d^2u.du,$$

m being arbitrary.

13. If u be the symbol of a surface, involving therefore two variable parameters, λ and μ suppose, then the symbol of the normal at the point u is

$$u + m\mathrm{D}\frac{du}{d\lambda}\cdot\frac{du}{d\mu},$$

m being arbitrary.

14. The symbol of the tangent plane at the point u is

$$u + mdu, \text{ or } u + m\frac{du}{d\lambda} + n\frac{du}{d\mu},$$

m and n being arbitrary.

15. The symbol of the plane which contains the three points $u\ u'\ u''$ is

$$u + m(u' - u) + n(u'' - u).$$

16. If u be the symbol of a right line, the symbol of the plane containing it and the point u' is

$$u + m(u' - u).$$

The following are examples of the proposed mechanical system in addition to those given in the paper already quoted.

1. If r be the radius vector of a planet, and $\alpha\ \beta\ \gamma$ be chosen so that α is the direction unit of the radius vector, and γ perpendicular to the plane of the orbit, it may be shown immediately by the symbolical method, that the symbol of the force acting on the planet is

$$\left(\frac{d^2r}{dt^2} - r\omega^2\right)\alpha + \frac{1}{r}\frac{d(r^2\omega)}{dt}\beta + r\omega\omega'\gamma,$$

where ω is the angular velocity of the planet, and ω' that of the plane of the orbit about the radius vector. The expressions for the three component forces along r, perpendicular to r, and perpendicular to the plane of the orbit, are the coefficients of $\alpha\ \beta\ \gamma$ in this expression.

2. The equation of motion of the planet, when the force is the attraction of a fixed centre varying as the inverse square of the distance, is

$$\frac{d^2u}{dt^2} = -\frac{\mu\alpha}{r^2}.$$

It is curious that this equation is *immediately* integrable, the integral being the two equations

$$r^2\omega = h;$$

$$\frac{h}{r} = \frac{\mu}{h} + e\Delta\beta.s.$$

The latter equation is the symbolical equation of a conic section,

the origin being focus, h c and ε being the arbitrary constants introduced by integration.

3. The application of this method to the case of a planet acted on by a disturbing force is worthy of particular notice, as it expresses the variations of the elements of the orbit with great facility, in the following manner:—

If U be the symbol of the disturbing force, we have

$$\frac{d(h\gamma)}{dt} = \mathrm{D}u.\mathrm{U} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

$$\frac{d(e\varepsilon)}{dt} = \frac{\mu r}{h^2}\beta\Delta\beta.\mathrm{U} + \mathrm{U}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

These two equations determine with great facility all the elements of the orbit. For γ is a direction unit perpendicular to the plane of the orbit (*i. e.* it is the symbol of the pole of the orbit), and therefore it defines completely the position of the plane of the orbit. Also ε is a direction unit in the plane of the orbit at right angles to the axis major, and therefore it determines the position of the axis major; in fact the direction unit of the axis major is $\mathrm{D}\gamma.\varepsilon$. The letters h and e have their usual signification.

To find h and γ separately from (1.), suppose that we obtain by integration of (1.)

$$h\gamma = \mathrm{W};$$

then $h^2 = \Delta\mathrm{W}.\mathrm{W}$; and h being thus found, we have $\gamma = \frac{\mathrm{W}}{h}$. The same observation applies to (2.).

4. The expression for the parallax of the planet is

$$\frac{\mu}{h^2} + \Delta\beta.\int dt\left(\frac{\mu r}{h^2}\beta\Delta\beta.\mathrm{U} + \mathrm{U}\right).$$

These instances suffice to show the nature of the proposed symbolical method.

PROCEEDINGS

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March 1, 1847.

On the Partitions of Numbers, on Combinations, and on Permutations. By Henry Warburton, M.P., F.R.S., F.G.S., Member of the Senate of the University of London; formerly of Trinity College, A.M.

The use made by Waring of the Partitions of numbers in developing the power of a polynome, induced the author to seek for some general and ready method of determining in how many different ways a given number can be resolved into a given number of parts. On his communicating the method described in article 5 of Section I. of this abstract, to Professor De Morgan, in the autumn of 1846, that gentleman intimated a wish that the author would turn his attention also to Combinations; and such was the origin of the researches which form the subject of the 2nd and 3rd sections.

I. *On the Partitions of Numbers.*

1. Let $[N, p_\eta]$ denote how many different ways there are of resolving the integer N into p integral parts, none less than η. Then

$$[N, p_\eta] = [N \pm p\theta, p_{\eta \pm \theta}]. \quad . \quad . \quad . \quad . \quad . \quad (\text{I.})$$

2. Such of the p-partitions of N as contain η as a part, and no part less than η, are obtained by resolving $N-\eta$ into $p-1$ parts not less than η, and by adding η, as a pth part, to every such $(p-1)$-partition. That is,

$$[N, p_\eta] - [N, p_{\eta+1}] = [N-\eta, \underset{\eta}{p-1}]. \quad . \quad . \quad . \quad (\text{II.})$$

3. In (II.), substitute $\eta+1$, $\eta+2$, &c. successively for η. The sum of the results is

$$[N, p_\eta] - [N, p_{\eta+\theta+1}] = \underset{s}{S}{}_0^\theta [N-\eta-sp, \underset{\eta}{p-1}]. \quad . \quad (\text{III.})$$

In this expression, when $\theta = I^*\left(\frac{N}{p}\right) - \eta$, the term $[N, p_{\eta+\theta+1}]$ vanishes, and the formula then becomes analogous to one published

* $I\left(\frac{N}{p}\right)$ is employed to avoid the long phrase, "the integer nearest to and not exceeding $\frac{N}{p}$."

anonymously by Professor De Morgan in a paper printed in the fourth volume, p. 87, of the Cambridge Mathematical Journal.

4. In (II.), for $[N, p_{\eta}+1]$ substitute $[N-p\eta, p_1]$, and transpose the terms. Then

$$[N, p_\eta]-[N-\eta, p\underset{\eta}{-}1]=[N-p\eta, p_1]; \quad . \quad . \quad \text{(IV.)}$$

and this leads to

$$[N-\eta, p\underset{\eta}{-}1]-[N-2\eta, p\underset{\eta}{-}2]=[N-p\eta, p\underset{\eta}{-}1];$$

and that leads to the summation

$$[N, p_\eta]=\underset{z}{S}{}_0^p[N-p\eta, z_1]. \quad . \quad . \quad . \quad . \quad \text{(V.)}$$

The lower limit of z in (V.) is made 0, in order that the formula may comprehend the extreme case $[0, 0_1]=1$, analogous to the extreme case in Combinations.

5. After substituting 1 for η, the author applies formula (IV.) to determining in how many different ways N can be resolved into p parts not less than 1. Let $[N, p_1]$ be the term in a table of double entry corresponding to column N, line p, in the table. From the head, in line 0, of each of the columns 0, 1, 2, 3, &c., draw a diagonal, advancing one column and one line at a time. Take these diagonals one after another, and in each of them compute by formula (IV.) the terms situate on lines 0, 1, 2, 3, &c., one by one in succession. If N be the number at the head of the column from which any diagonal takes its departure, there will be only N terms to compute on that diagonal, the further terms being only repetitions of the term on the line N. For the diagonal in question intersects line N in column 2N; and, by formula V,

$$[2N, N_1]=\underset{z}{S}{}_0^N[N, z_1]$$

= the sum of all the terms in column N. But, moreover,

$$[2N+y, N_1+y]=\underset{z}{S}{}_0^N[N, z_1]$$

= the same constant. The leading property of the table, indicated by the formula

$$[N, p_1]=\underset{z}{S}{}_0^p[N-p, z_1],$$

is, that the term $[N, p_1]=$ the sum of all the terms in column $N-p$, from line 0 to line p inclusive. After the publication of the anonymous paper before referred to, Professor De Morgan discovered this theorem also, but he did not announce it*.

II. *On Combinations.*

1. In ordinary Combinations, the combining elements are of different kinds, and there is but one element of a kind: in the case here considered, there are different kinds of elements, and there may be many elements of a kind; and more than one element of a kind may enter into the same combination.

* The author has recently discovered an equivalent formula in p. 264 of Euler's Int. in An. Infinitorum; but investigated by a totally different method, and not applied as the author has applied it.

2. If u elements enter at a time into each combination, and the *kinds* are determinate in number, and their number is s, let $\left\{ {u \atop s} \right\}$ denote how many different combinations can then be formed: if the *elements* are determinate in number, and their number is σ, let the number of the combinations which can then be constructed, be denoted by $\{u, \sigma\}$. If $\phi(x)$ be any function of x, let $D^u \phi(x)$ denote the coefficient of x^u in that function developed according to the powers of x.

3. The same things as before being assumed, let a given set of elements consist of α elements of the kind A, $+\beta$ elements of the kind B, + &c. Take the product, K, of the s geometrical progressions,

$[1+Ax+A^2x^2+\ldots.+A^\alpha x^\alpha]$, $[1+Bx+B^2x^2+\ldots.+B^\beta x^\beta]$,&c.

Then K will be of the form,

$$1+S[A]x+S[A^2+AB]x^2+S[A^3+A^2B+ABC]x^3+ \text{\&c.},$$

and $D^u[K]$ will be of the form

$$S[A^pB^qC^r \text{ \&c.}],$$

the last expression being an aggregate of terms of the form $A^pB^qC^r\ldots$, each containing a different combination of u of the given elements, and their sum comprehending all the possible combinations of those elements taken u at a time. Now, if A, B, C, &c. be each made equal to 1, K will become

$$k=[1+x+x^2+\ldots+x^\alpha][1+x+x^2+\ldots+x^\beta]. \text{ \&c.};$$

each of the terms $A^p.B^q.C^r$. &c. will become 1, and the *number* of all the terms of the form $A^pB^qC^r\ldots$ which $D^u[K]$ or $S[A^pB^qC^r\ldots]$ contains, that is to say, $\{u, \sigma\}$ will be represented by $D^u[k]$; which latter coefficient the author next proceeds to determine.

Now

$$\left.\begin{aligned} k=\frac{1-x^{\alpha+1}}{1-x}.\ \frac{1-x^{\beta+1}}{1-x}.\ \text{\&c.} &=[1-x^{\alpha+1}][1-x^{\beta+1}]..[1-x]^{-s} \\ &=[1-x^{\alpha+1}][1-x^{\beta+1}]\ldots \mathop{S}_{u}{}_0^\infty \left[\frac{s^{u|1}}{1^{u|1}}{}^{*}x^u\right] \end{aligned}\right\}\text{(VI.)}$$

$$=[1-x^{\alpha+1}][1-x^{\beta+1}]\ldots\frac{1}{1^{s-1|1}}\mathop{S}_{u}{}_0^\infty\left[[u+1]^{s-1|1}x^u\right]\text{(VII.)}$$

For brevity, write u_1, α_1, β_1, &c. respectively, for $u+1$, $\alpha+1$, $\beta+1$, &c.; and also write [1] for $[1-x]^{-s}$; [2] for $[1-x^{\alpha_1}][1-x]^{-s}$; that is, for $[1-x^{\alpha_1}].[1]$; [3] for $[1-x^{\alpha_1}][1-x^{\beta_1}][1-x]^{-s}$; that is, for $[1-x^{\beta_1}].[2]$, and so on. Then

$$D^u[2]=D^u[1]-D^{u-\alpha_1}[1];$$

and

$$D^u[3]=D^u[2]-D^{u-\beta_1}[2];$$

* According to the factorial notation, here used by the author, $s^{u|\pm1}$ represents $s\,[s\pm1][s\pm2]\ldots[s\pm(u-1)]$.

and

$$D^u[4]=D^u[3]-D^{u-\gamma_1}[3]\text{; and so on;} \quad \text{(VIII.)}$$

and the developed product of the binomes,

$$[1-x^{\alpha_1}],[1-x^{\beta_1}],[1-x^{\gamma_1}],\text{\&c.};$$

that is to say,

$$\begin{aligned} &1-x^{\alpha_1}+x^{\alpha_1+\beta_1}-x^{\alpha_1+\beta_1+\gamma_1}+\text{\&c.}\\ &\quad -x^{\beta_1}+x^{\alpha_1+\gamma_1}-\text{\&c.}\\ &\quad -x^{\gamma_1}+x^{\beta_1+\gamma_1}\\ &\quad -\text{\&c.}+\text{\&c.}\end{aligned}$$

when multiplied into the development of $[1-x]^{-s}$, manifestly leads to the following formula:

$$\left.\begin{aligned} D^u[m]=D^u[1]-S\left[D^{u-\alpha_1}[1]\right]+S\left[D^{u-\alpha_1-\beta_1}[1]\right]\\ -S\left[D^{u-\alpha_1-\beta_1-\gamma_1}[1]\right]+\text{\&c.}\qquad\end{aligned}\right\}\text{(VIII*.)}$$

where, since the powers of x, in (VI.) or (VII.) developed, are to be all positive, no expression of the form

$$(u-\alpha_1),\ (u-\alpha_1-\beta_1),\ (u-\alpha_1-\beta_1-\gamma_1),\ \text{\&c.}$$

is to be negative. Then by giving to

$$D^u[1],\ D^{u-\alpha_1}[1],\ D^{u-\alpha_1-\beta_1}[1],\ \text{\&c.} \quad . \quad . \quad . \quad \text{(IX.)}$$

their respective values, we obtain the series of expressions:

$$D^u[1]=\frac{1}{1^{s-1|1}}u_1^{s-1|1}=\frac{s^{u|1}}{1^{u|1}}=\begin{bmatrix}u\\ s\end{bmatrix}$$

where in all the kinds the elements are plural without limit; a formula given by Hirsch:

$$D^u[2]=\frac{1}{1^{s-1|1}}\left[u_1^{s-1|1}-[u_1-\alpha_1]^{s-1|1}\right]=\begin{bmatrix}u\\ s\end{bmatrix}$$

where the elements A are limited in number to α, but those of the other $(s-1)$ kinds are plural without limit:

$$D^u[3]=\frac{1}{1^{s-1|1}}\begin{bmatrix}u_1^{s-1|1}-[u_1-\alpha_1]^{s-1|1}+[u_1-\alpha_1-\beta_1]^{s-1|1}\\ -[u_1-\beta_1]^{s-1|1}\end{bmatrix}=\begin{bmatrix}u\\ s\end{bmatrix}$$

where, moreover, the elements B are limited in number to β, but those of the other $(s-2)$ kinds are plural without limit: and so for the rest. The law of the terms being evident, they need not be continued further.

Example of (IX.). Given one element of 1 kind, two elements of a 2nd kind, three of a 3rd, and four of a 4th; and let $u=5$. Then

$$\{u,\sigma\}=\frac{1}{1.2.3}\left[6.7.8\begin{matrix}-4.5.6\\ -3.4.5\\ -2.3.4\\ -1.2.3\end{matrix}+1.2.3\right]=22.$$

4. If $\alpha=\beta=\gamma=$&c., formula (IX.) becomes

$$\{u, \sigma\}=\frac{1}{1^{s-1|1}}\; \underset{\theta\quad 0}{\mathrm{S}^{1}}\left(\frac{u_1}{\alpha_1}\right)\left[(-1)^{\theta}\frac{s^{\theta|-1}}{1^{\theta|1}}\left[u_1-\theta\alpha_1\right]^{s-1|1}\right]. \qquad \text{(X.)}$$

Example of formula (X.) Given seven kinds of elements, and three of each kind; and let $u=4$. Then

$$\{u, \sigma\}=\frac{1}{1.2.3.4.5.6}\Big[5.6.7.8.9.10-7.1.2.3.4.5.6\Big]=203.$$

5. If it is required to determine many, or all, of the terms of the series $\{0,\sigma\}$, $\{1,\sigma\}$, $\{2,\sigma\}$, ..., $\{\sigma,\sigma\}$, formulas (VIII.) suggest the following process for the determination of those terms. An example will best explain the process.

Given 1 element of one kind, 2 elements of a second kind, and 3 of a third kind. How many combinations can be formed from these elements, when taken 0, 1, 2, 3, 4, 5, 6 at a time, respectively?

Values of u	0	1	2	3	4	5	6
Coefficients of x^u in $[1-x]^{-3}$	1	3	6	10	15	21	28
Multiply by $[1-x^2]$; that is, subtract	...	...	1	3	6	10	15
Coefficients of x^u in $[1-x^2][1-x]^{-3}$	1	3	5	7	9	11	13
Multiply by $[1-x^3]$; that is, subtract	...	...	...	1	3	5	7
Coefficients of x^u in $[1-x^2][1-x^3][1-x]^{-3}$	1	3	5	6	6	6	6
Multiply by $[1-x^4]$; that is, subtract	...	...	...	...	1	3	5
Coefficients of x^u in $[1-x^2][1-x^3][1-x^4][1-x]^{-3}$	1	3	5	6	5	3	1
That is	$\{0,\sigma\}$	$\{1,\sigma\}$	$\{2,\sigma\}$	$\{3,\sigma\}$	$\{4,\sigma\}$	$\{5,\sigma\}$	$\{6,\sigma\}$

6. Let a set of elements, S, such as we have been previously considering, consist of two similar sets, T and T′, which do not contain in common any elements of the same kind. If S consists of σ elements combining u at a time, and T consists of τ elements combining v at a time, T′ will consist of $(\sigma-\tau)$ elements combining $(u-v)$ at a time. Consider u as constant, for the moment, and v as variable. The author then shows that if by the process described in art. 5, the whole series of terms $\{v,\tau\}$ and the whole series of terms $\{u-v, \sigma-\tau\}$, have been determined, we can thence determine the whole series of terms $\{u, \sigma\}$ by means of the formula

$$\{u, \sigma\}=\underset{v\;0}{\mathrm{S}^{\tau}}\Big[\{v, \tau\}.\{u-v, \sigma-\tau\}\Big]; \qquad . \quad . \quad \text{(XI.)}$$

and of this he gives examples.

7. In formula (XI.) substitute $(\sigma-u)$ for u; and develope $\{u, \sigma\}$ and $\{\sigma-u, \sigma\}$ in the manner indicated by that formula. By comparing the 1st, 2nd, 3rd, &c. terms respectively of $\{u, \sigma\}$ with the last, last but one, last but two, &c. terms of $\{\sigma-u, \sigma\}$, and *vice*

versâ, the author shows that $\{u, \sigma\}$ will be identical with $\{\sigma - u, \sigma\}$, provided $\{v, \tau\}$ is identical with $\{\tau - v, \tau\}$, and provided also $\{u - v, \sigma - \tau\}$ is identical with $\{\sigma - \tau - (u - v), \sigma - \tau\}$. But this identity actually exists when T consists of elements of one kind only, and when T′ also consists of elements of one kind only. For, in that case, every term of the series $\{v, \tau\}$ and every term of the series $\{u - v, \sigma - \tau\}$ is equal to 1. Let the elements of the single kind which T contains, be different from those of the single kind which T′ contains. Then the identity in question will exist, when S consists of elements, finite in number, of two different kinds: consequently, it exists also when T consists of elements, finite in number, of two different kinds, and T′ consists of elements, finite in number, of one or two other kind or kinds; that is, when S consists of elements, finite in number, of three or four different kinds. And therefore universally, in the case as well of finitely plural, as of singular elements, the following law obtains:

$$\{u, \sigma\} = \{\sigma - u, \sigma\} \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(XII.)}$$

Hence it follows that in applying formulas (IX.) and (X.) to particular cases, the labour of computation will be shortened by substituting for the variable the lesser of the two numbers u and $\sigma - u$.

8. The author next considers how many different combinations can be formed from a given set of elements, when every combination is to be constructed in conformity with a given type; in which type there are m different kinds containing v elements each, m' other different kinds containing v' elements each, m'' other different kinds containing v'' elements each, and so on; and where, consequently, in each combination, z, the number of kinds, is $m + m' + m'' +$ &c.; and u, the number of elements, is $mv + m'v' + m''v'' +$ &c. The type remaining constant, any combination conformable thereto may be altered, either by changing the particular z kinds which are selected out of the s given kinds; or, the kinds remaining the same, by altering the distribution of the parts $v, v, v, \ldots (m) v', v', v', \ldots (m') v'', v'', v'', \ldots (m'')$ &c., among those kinds. When all the elements are plural without limit, the changes of the former description will be represented by

$$\frac{s^{z|-1}}{1^{z|1}};$$

and those of the latter description by

$$\frac{1^{z|1}}{1^{m|1} \cdot 1^{m'|1} \cdot 1^{m''|1} \ldots},$$

and their joint effect by the product

$$\frac{s^{z|-1}}{1^{z|1}} \times \frac{1^{z|1}}{1^{m|1} \cdot 1^{m'|1} \cdot 1^{m''|1} \ldots} \quad . \quad . \quad . \quad . \quad \text{(XIII.)}$$

But when the elements of all the given kinds are finite in number, class these kinds, so that each kind in class 1 contains not fewer

than v elements; each kind in class 2 contains fewer than v, but not fewer than v' elements; each kind in class 3 contains fewer than v', but not fewer than v'' elements; and so on; and so that the given kinds may in this way be reduced, say, to t kinds containing v elements each $+ T'$ kinds containing v' elements each $+ T''$ kinds containing v'' elements each, &c. Then let $t-m+T'=t'$; $t'-m'+T''=t''$; and so on. The given kinds being thus ordered, since we are required to select, 1st, m out of t kinds; then, 2nd, m' out of t' kinds; then, 3rd, m'' out of t'' kinds; and so on; the number of the different combinations which can be constructed from those kinds in conformity with the type, will be

$$\frac{t^{m|-1}}{1^{m|1}} \cdot \frac{{}'t^{m'|-1}}{1^{m'|1}} \cdot \frac{{}''t^{m''|-1}}{1^{m''|1}}, \text{ \&c.} \quad . \quad . \quad . \quad . \quad \text{(XIV.)}$$

If $tv+T'v'+T''v''$.. &c. is reduced to a single term, $t.v$; then formula (XIV.) becomes

$$\frac{t^{z|-1}}{1^{m|1}.\,1^{m'|1}.\,1^{m''|1}.} \text{ \&c.} \quad . \quad . \quad . \quad . \quad . \quad \text{(XV.)}$$

Example of (XIV.). Given eight elements of 1 kind, seven of a 2nd kind, six of a 3rd, five of a 4th, four of a 5th, three of a 6th, two of a 7th, and one element of an 8th kind, out of which it is required to construct combinations, each consisting of three kinds with five elements each + two kinds with three elements each + one kind with two elements. Of such combinations there can be formed

$$\frac{4^{3|-1}}{1^{3|1}} \cdot \frac{3^{2|-1}}{1^{2|1}} \cdot \frac{2^{1|-1}}{1^{1|1}} = 24.$$

9. If it be required to determine how many different combinations can be constructed, each containing u elements of z kinds, and the given elements are all finite in number; we must form all the different z-partitions of u; and each of these partitions being regarded as a type, we must determine, by formula (XIV.) or (XV.), how many combinations correspond to each of these types; and the total number required will be the sum of all these particular determinations. But if the given elements may all be repeated without limit, it follows from formula (XIII.), that the sum of all the particular determinations may be represented by

$$\frac{g^{z|-1}}{1^{z|1}} \times S\left(\frac{1^{z|1}}{1^{m|1}.\,1^{m'|1}.\,1^{m''|1}.\text{ \&c.}}\right).$$

Now

$$S\left(\frac{1^{z|1}}{1^{m|1}.\,1^{m'|1}.\,1^{m''|1}}\right)$$

denotes how many different permutations can be formed, when, in each different z-partition of u, the parts are permuted z together at a time; and the number of such permutations is

$$\Sigma^{z-1}[1]=\Sigma^{z-2}[u-1]=\frac{[u-1]^{z-1|-1}}{1^{z-1|1}}.$$

Consequently the required sum is

$$\frac{s^{z|-1}}{1^{z|1}} \times \frac{[u-1]^{z-1|-1}}{1^{z-1|1}} \quad . \quad . \quad . \quad . \quad . \quad \text{(XVI.)}$$

If in (XVI.) z varies from 0 to $u-1$,

$$\underset{z}{\mathrm{S}}{}_{0}^{u-1}\left[\frac{s^{z|-1}}{1^{z|1}} \times \frac{[u-1]^{z-1|1}}{1^{z-1|1}}\right] = \frac{s^{u|1}}{1^{u|1}},$$

this summation being a particular case of formula (XI.). The result agrees with $D^{u}[1]$ formula (IX.), art. 3.

10. When the given elements are all finite in number, we may determine $\{u, \sigma\}$, by taking the sum of all the particular determinations that may be obtained pursuant to art. 9, by giving to z the successive values 0, 1, 2, 3, &c. If $u < s$, the upper limit of z is u, and the number of types to be formed is $[2u, u_1]$; which becomes $[2s, s_1]$, if $u = s$. If $u > s$, the upper limit of z is s; and the number of types to be formed is $[u+s, s_1]$. (See articles 4 and 5, Section I.) But, if the repetition is finite, some of these partitions may fail to yield combinations.

11. If the elements A, B, C, &c. represent different prime numbers, all the methods and theorems contained in this section will apply, *mutatis mutandis*, to the composite numbers of which those primes, or the powers of those primes, are divisors.

III. *On Permutations*.

1. Let the given elements be of s different kinds. We can determine in two known cases, by an explicit function of u, when the elements are taken u at a time, in how many different ways they can be permuted. The number of the permutations is denoted, when there is but one element of a kind, by $s^{u|-1}$; and when in all the kinds the elements are plural without limit, by s^{u}. When the plurality is finite, it is only in the particular case of all the elements being permuted at a time, that there is a known formula to express the number of their permutations.

2. Every combination constructed on a given type, $u = mv + m'v' + m''v'' +$ &c., will generate the same number of permutations,

$$\frac{1^{u|1}}{[1^{v|1}]^{m}[1^{v'|1}]^{m'}[1^{v''|1}]^{m''}\,.\,\&c.} = \mathrm{P}.$$

Therefore, if the number of the different combinations which can be constructed out of the given elements in conformity with that type, is represented by Q, Q × P will be the number of the permutations corresponding to the type and to those elements. If the plurality be without limit,

$$\frac{s^{z|-1}}{1^{m|1}\,.\,1^{m'|1}\,.\,1^{m''|1}\,.\,\&c.} \times \mathrm{P}$$

will be the number of the permutations. If the given elements be finite in number, as in formulas (XIV.) and (XV.), the number of

the permutations corresponding to those elements and to the type, will be

$$\frac{t^{m|-1}}{1^{m|1}} \cdot \frac{'t^{m'|-1}}{1^{m'|1}} \cdot \frac{''t^{m''|-1}}{1^{m''|1}} \text{ \&c.} \times \text{P}.$$

Every different partition of u that may be formed within the limits pointed out in art. 10, Section II., will give rise to a similar product, $\text{Q}\times\text{P}$; and the sum of all these particular products, $\text{S}[\text{Q}\times\text{P}]$, will show how many different permutations can be formed from the given elements, taken u at a time. The author illustrates this method of computing the number of permutations, by examples.

3. Let $\text{P}\left\{\begin{matrix} u \\ s \end{matrix}\right\}$ denote how many different permutations can be formed when u elements are taken at a time out of s kinds; and $\text{P}\{u, \sigma\}$ denote how many different permutations can be formed when u elements are taken at a time out of σ, a finite number of elements. If all the elements may be repeated without limit,

$$\left\{\begin{matrix} u \\ s \end{matrix}\right\} = \text{D}^u[1^{u|1} . \varepsilon^{sx}] = s^u$$

$$= \text{D}^u\left[1^{u|1}\left[1 + x + \frac{x^2}{1.2} + \ldots . \frac{x^u}{1^{u|1}} + \ldots .\right]^s\right].$$

Hence the author infers that, if the elements A are limited in number to α, while those of the other $(s-1)$ kinds are plural without limit,

$$\text{P}\left\{\begin{matrix} u \\ s \end{matrix}\right\} = \text{D}^u\left[1^{u|1}\, \varepsilon^{(s-1)x}\left[1 + x + \frac{x^2}{1.2} + \ldots + \frac{x^\alpha}{1^{\alpha|1}}\right]\right];$$

that if, moreover, the elements B are limited in number to β, while the other $(s-2)$ kinds are plural without limit,

$$\text{P}\left\{\begin{matrix} u \\ s \end{matrix}\right\} = \text{D}^u\left[1^{u|1}\, \varepsilon^{(s-2)x}\left[1 + x + \frac{x^2}{1.2} + \ldots \frac{x^\alpha}{1^{\alpha|1}}\right]\right.$$
$$\left.\left[1 + x + \frac{x^2}{1.2} + \ldots . \frac{x^\beta}{1^{\beta|1}}\right]\right];$$

and so on, until finally, if all the elements are finite in number, and the elements A, B, C, &c. are respectively limited, in point of number, to α, β, γ, &c.,

$$\left.\begin{aligned} \text{P}\{u, \sigma\} = \text{D}^u\Big[1^{u|1}\Big[1 + x + \ldots \frac{x^\alpha}{1^{\alpha|1}}\Big]\Big[1 + x + \ldots \frac{x^\beta}{1^{\beta|1}}\Big] \\ \Big[1 + x + \ldots \frac{x^\gamma}{1^{\gamma|1}}\Big]\Big]. \end{aligned}\right\} . \quad \text{(XVII.)}$$

4. Hence, if in all the s kinds the elements are dual, (XVII.) becomes

$$\left.\begin{array}{l} P\{u, \sigma\} = D^{u}\left[1^{u|1}\left[1+x+\frac{x}{2}\right]^{s}\right] = \\ \qquad \underset{\theta}{S}{}_{0}^{I\left(\frac{u}{2}\right)}\left[\frac{u^{2\theta|-1} \times s^{u-\theta|-1}}{2^{\theta|2}}\right]. \end{array}\right\} \quad . \quad \text{(XVIII.)}$$

This is the only addition which the author has been able to make to the cases wherein $P\begin{bmatrix} u \\ s \end{bmatrix}$, or $P\{u, \sigma\}$ is expressed by an explicit function of u, symmetrical in form.

Example. Let there be five kinds of elements, and two of each kind. Let $u=3$.

$$P\{u, \sigma\} = \frac{1.5.4.3}{1} + \frac{3.2 \times 5.4}{2} = 120.$$

5. The author gives the following theorem, which is precisely analogous to that of art. 6, Sect. II., formula (XI.), in Combinations; viz.

$$P\{u, \sigma\} = \underset{v}{S}{}_{0}^{\tau}\left[\frac{u^{v|-1}}{1^{v|1}} P\{v, \tau\} . P\{u-v, \sigma-\tau\}\right]. \qquad \text{(XIX.)}$$

6. By a mode of proof precisely analogous to that employed in art. 7, Sect. II., he shows that $P\{\sigma-1, \sigma\} = P\{\sigma, \sigma\}$; that is to say, that

$$\frac{1^{\sigma|1}}{1^{\alpha|1} . 1^{\beta|1} . 1^{\gamma|1} . \&c.}$$

denotes the number of permutations that can be formed with α elements A, β elements B, &c. (where $[\alpha+\beta+\gamma+ \&c.] = \sigma$), as well when $\sigma-1$ elements, as when σ elements, are taken at a time.

Since correcting his paper for publication, the author has had his attention called to the work of Bézout on Elimination (4to. Paris, 1779, p. 469), as containing a formula similar in structure to that numbered VIII*. in the present abstract.

Bézout investigates the composition of a polynome function of s quantities, A, B, C, &c., consisting of terms which are of the form $A^p B^q C^r$, and of every dimension from 0 to u inclusive. Let $[s]^u$ denote such a polynome, complete in all its terms, and $N[s]^u$ the number of its terms. Then, 1st,

$$N[s]^u = \frac{[u+1]^{s|1}}{1^{s|1}};$$

and 2nd, the number of the terms in $[s]^u$ which are not divisible by either A^α, or B^β, or C^γ, &c., he expresses by

$$\begin{array}{l} N[s]^u - N[s]^{u-\alpha} + N[s]^{u-\alpha-\beta} - \&c. \\ \qquad - N[s]^{u-\beta} + \&c. \\ \qquad - \&c. \end{array}$$

He also observes (p. 39) that when A^{α}, B^{β}, C^{γ}, &c. are the highest powers of A, B, C, &c. which a polynome, agreeing in other respects with $[s]^{u}$, contains, the terms of such incomplete polynome will agree in point of number with those terms in $[s]^{u}$ which are not divisible by either $A^{\alpha+1}$, or $B^{\beta+1}$, or $C^{\gamma+1}$, &c. The polynomes from which Bézout proposes to eliminate certain terms, contain terms of all dimensions from 0 to u inclusive. The terms which are to remain after the others have been eliminated, and which are enumerated by means of the condition, that they are not divisible by certain powers of A, or B, or C, &c., may be of all dimensions indiscriminately from 0 to u inclusive. Bézout's object is exclusively Elimination, and he makes no allusion to any other application of his formulæ.

The polynomes considered by the author, taken in their entirety, agree in their general structure with those considered by Bézout; but the nature of the author's inquiries led him to confine his attention to the composition of those particular terms in a polynome which were of the same dimension; and to seek to express the number of the terms, not of all dimensions indiscriminately, but of each particular dimension separately. To show how it has happened that researches, very different at their point of departure, have, as regards one point of investigation, ended in nearly similar formulæ, the author proceeds to deduce his formula (VIII*.) from the investigations of Bézout. Such a deduction, he conceives, might readily have been made by any one to whom it had occurred to make it; and the application of such a deduction, when once made, to problems in Combinations, would have been much too obvious to have remained long unnoticed.

Expressions of the form above considered are regarded by Bézout as of the nature of Differences; and the truth of this view of the subject may be shown in the following brief manner.

If $\phi(x)$ generates $\psi(u)$, $[1-x^{\alpha}]\phi(x)$ will generate $\psi(u)-\psi(u-\alpha)$, which we may denote by $\Delta_{\alpha}\psi(u)$. Consequently $[1-x^{\beta}][1-x^{\alpha}]$ $\phi(x)$, that is to say,

$$\left\{\begin{matrix} 1-x^{\alpha}+x^{\alpha+\beta} \\ -x^{\beta} \end{matrix}\right\}\phi(x)$$

will generate $\Delta^{2}_{\alpha,\beta}\psi(u)$; and so on; the independent variable, u, undergoing, not uniform, but variable decrements, α, β, γ, &c.

May 3, 1847.

On the Internal Pressure to which Rock Masses may be subjected, and its possible influence in the Production of the Laminated Structure. By W. Hopkins, M.A., F.R.S.

If a plane of indefinitely small extent pass through any proposed point in the interior of a continuous solid mass in a state of con-

straint, the resultant pressure or tension on this plane will vary with the angular position of the plane, and its direction will not, as in fluid masses, be generally perpendicular to the plane. There are, however, three angular positions in which the direction of the pressure does coincide with a perpendicular to the plane. These are called *principal directions*, and are at right angles to each other; the corresponding pressures are called *principal pressures*. In these particular positions of the plane there will be no *tangential* action upon it; but generally the whole pressure or tension may be resolved into two parts, of which one is *normal* and the other *tangential*. In certain positions of the plane these forces assume their maximum or minimum values. The normal action is a maximum, when a perpendicular to the plane coincides with one of the three principal directions; and a minimum, when it coincides with another, the third of those directions, not corresponding either to a maximum or minimum value. These conclusions have been established by Poisson, Cauchy and others. In this paper the author has investigated the positions of the small plane, when the *tangential force* upon it is a maximum. There are two of these positions perpendicular to each other, in each of which the plane passes through that principal direction which does not correspond to either the maximum or minimum value of the normal force, and bisects the corresponding right angle between the other two principal directions—those of the maximum and minimum normal forces. Having established the relative positions of the planes of greatest normal and of greatest tangential action, the author proceeds to examine how far the evidence afforded by the distorted forms of organic remains may justify the conclusion that these forces have had an influence in determining the position of the planes of cleavage in the rocks containing those remains.

Conceive one stratified bed placed on another, and acted on by forces tending to give the upper a small sliding motion along the surface of the lower one. A considerable *tangential force* will be called into action between the beds; and if any object be placed between them, its lower part will be pushed in one direction by the action of the lower bed, while its upper part will be equally pushed in the opposite direction by the action of the upper bed, and thus the object will be *twisted* from its original form. For example, suppose the object be an equilateral shell lying between the two beds, with the plane of junction of the two valves parallel to the surfaces of the beds, and suppose the median line of either valve to be perpendicular to the direction in which the one bed tends to move along the other. The shell in its distorted form will no longer be equilateral; one half of each shell will be crumpled into a smaller space, while the other half will be extended into greater breadth; so that if there be longitudinal folds on the valve, those on the former half will be pressed together, and those on the latter will be dilated into greater breadth. An exactly similar effect will be produced on both shells; but the compressed half of one will be opposite to the dilated half of the other.

Again, suppose the beds to be acted on by forces tending to com-

press them equally in a direction parallel to their surfaces. The shell will then be *compressed* in the same direction, so that, generally, the ratio of the length to the breadth of the shell will be altered, but without that *twisting* which will characterize the distorted form in the former case. In the case of this paragraph, the direction of compression will coincide with what has been above termed a *principal direction*, and it will also be that of *maximum normal pressure*. In the previous case, the common surface of the two beds will be the plane of *maximum tangential action*.

If, then, in any stratified mass, we observe the organic remains to be regularly distorted, and *twisted* from their original forms, as above described, we may conclude that the planes of stratification have nearly coincided with those of *maximum tangential action*; but if, on the contrary, the distortion consists only in compression of the shells in a given direction along the surface of the bed where they are found, we may conclude that the direction of *maximum normal pressure* has nearly coincided with this direction of compression, and was consequently parallel to the planes of stratification. The masses in which distorted remains have been found, are generally those which have been much disturbed. The disturbing forces are those to which the distortions are to be referred; and it may be remarked, that in such cases the directions of maximum and minimum pressure at any point would probably lie in a plane perpendicular to the strike of the elevated beds, and that consequently the planes of maximum tangential action, which bisect the angles between those directions, will have approximately the same strike as the beds themselves.

The bearing of these conclusions on the question of laminated structure is easily seen. Suppose the planes of lamination are observed to be nearly coincident with those of stratification, and that the distortion of the organic remains consists in their being *twisted* from their primitive forms. Then, if the position of the planes of lamination has been due to the internal pressures to which the mass has been subjected, it is to *tangential action*, and not to *direct pressure*, that the effect is attributable. Again, if the planes of lamination have nearly the same strike as the beds, and are inclined to them at an angle of about 45°, while the organic remains have been distorted only by *direct compression*, the planes of lamination must in this case also have coincided with those of maximum tangential action, and we shall have the same conclusion as in the former case. The direction of compression of the organic forms ought, according to this view, to be perpendicular to the intersections of the planes of lamination and those of stratification.

Mr. Sharpe, in a paper recently published in the Journal of the Geological Society, has stated nearly all the evidence hitherto collected on this subject; and it appears that the organic bodies are most *twisted* from their original forms in those cases in which the planes of lamination coincide most nearly with those of stratification, and that they have generally suffered most *direct compression* without twisting in those cases in which the planes of lamination are inclined to those of stratification at an angle of 40° or 50°. We must

therefore conclude, according to the last paragraph, that *the planes of lamination approximately coincide with those which were formerly the planes of greatest tangential action.*

The author does not regard this mechanical action as the probable primary cause of the laminated structure, but rather as a secondary cause, which may have had its influence in determining the positions of the planes of lamination. He trusts that further evidence will be collected on the subject.

May 17, 1847.

On the Symbolical Equation of Vibratory Motion of an Elastic Medium, whether Crystallized or Uncrystallized. By the Rev. M. O'Brien, late Fellow of Caius College, Professor of Natural Philosophy and Astronomy in King's College, London.

The object of the author in this paper is twofold: *first*, to show that the equations of vibratory motion of a crystallized or uncrystallized medium may be obtained in their most general form, and very simply, without making any assumption as to the nature of the molecular forces; and *secondly*, to exemplify the use of the symbolical method and notation explained in two papers read before the Society during the present academical year.

First, with regard to the method of obtaining the equations of vibratory motion.

This method consists in representing the *disarrangement* (or state of relative displacement) of the medium in the vicinity of the point xyz by the equation

$$\delta v = \frac{dv}{dx}\delta x + \frac{dv}{dy}\delta y + \frac{dv}{dz}\delta z + \frac{1}{2}\frac{d^2v}{dx^2}\delta x^2 + \frac{d^2v}{dxdy}\delta x\delta y + \&c. - \&c.$$

(where $v=\xi\alpha+\eta\beta+\zeta\gamma$, $\xi\eta\zeta$ denoting, as usual, the displacements at the point xyz, and $\alpha\beta\gamma$ being the *direction units* of the three coordinate axes), and in finding the *whole* force brought into play at the point xyz (in consequence of this disarrangement) by the *symbolical addition* of the different forces brought into play by the several terms of δv, *each considered separately*. It is easy to see that these different forces may be found with great facility, without assuming anything respecting the constitution of the medium more than this, that it possesses *direct* and *lateral elasticity*. By *direct elasticity* we mean that elasticity in virtue of which *direct* or *normal* vibrations take place; and by *lateral*, that in virtue of which *lateral* or *transverse* vibrations take place.

The forces due to the several terms of δv are obtained by means of the following simple considerations.

Let AB be any line in a perfectly uniform medium, and conceive the medium to be divided into elementary slices by planes perpendicular to AB; let OM($=x$) be the distance of any slice PP′ from any particular point O of AB, and suppose this slice to suffer a dis-

placement equal to $\frac{1}{2}cx^2$ (c being a constant) *in* the direction OAB, and the other slices to be similarly displaced. Then it is evident that the medium suffers by these displacements a uniformly increasing *expansion* in the direction OB, and a uniformly increasing condensation in the direction OA; the rate of increase both of the expansion and condensation being c. Now in all known substances, whether solid, fluid, or gaseous, a disarrangement of this kind would bring into play on the slice O a force along the line AB proportional to the rate of increase c, *i. e.* a force Ac, A being a constant depending upon what we may call the *direct elasticity* of the substance.

Again, suppose that the slice PP′ receives a displacement $\frac{1}{2}cx^2$ in the direction OC *perpendicular* to AB, and the other slices similar displacements. Then the line AB will become curved into a parabola A′OB′, and all the lines of the medium parallel to AB will be similarly curved, the radius of curvature being equal to $\frac{1}{c}$ and perpendicular to AB. Now in all known substances* a disarrangement of this kind would bring into play upon the slice O a force in the direction OC proportional to the curvature c, *i. e.* a force Bc depending upon what we may call the *lateral elasticity* of the substance.

Lastly, suppose that MP$=y$, and that the point P of the medium receives a displacement cxy parallel to AB, and the other points similar displacements. Then the slice PP′ will, in consequence of this kind of displacement, turn through an angle $\tan^{-1}(cx)$ into the dotted position, and the other slices will suffer similar rotations, those on the other side of O, such as QQ′, turning the opposite way. Now it is easy to see that a disarrangement of this kind produces a uniformly increasing expansion in the direction OC, and a uniformly increasing condensation in the direction OC′, the rate of increase both of the expansion and condensation being c. But the expansion and condensation here described are quite different from that previously noticed; since it is produced, not by displacements parallel to C′C, but by *lateral* displacements, *i. e. perpendicular* to C′C. On this account all that we can assert without further investigation is, that the force brought into play upon an element at O by this disarrangement acts along the line C′C, and is proportional to c, *i. e.* equal to Cc, where C is some constant evidently depending in some way both upon the *direct* and *lateral* elasticity of the medium.

There is however a very simple way of finding the precise value of the force brought into play by a disarrangement of this kind; for if we turn the axes of x and y in the plane of the paper through an angle of 45°, it appears that this disarrangement is nothing but a combination of the two kinds of disarrangement previously noticed; and from this it immediately follows, in the case of an uncrystallized medium, that the force brought into play at O is $(A-B)c$; in other

* Fluids and gases possess lateral elasticity as well as solids, only in a comparatively feeble degree.

words, the coefficient C, which must be multiplied into c, in order to give the force brought into play by the disarrangement cxy, is equal to the coefficient of direct elasticity (A) minus the coefficient of lateral elasticity (B).

In the case of a crystallized medium, it may be shown that *six relations*, corresponding to the relation $C=A-B$, are most probably true, and are *essential* to Fresnel's theory of transverse vibrations; that is to say, the medium is capable of propagating waves of transverse vibrations if these six conditions hold, but otherwise it is not.

In employing the above considerations to determine the equations of vibratory motion, the directions AB and C′C are always taken so as to coincide with some two of the three coordinate axes; and it is this circumstance that makes the method peculiarly applicable to crystallized media. Indeed, if it were necessary to take the lines AB and C′C in any directions but those of the axes of symmetry, the above considerations would not apply without considerable modification.

The equations of vibratory motion obtained by this method for an uncrystallized medium, are the well-known equations involving the two constants A and B. The equations obtained for a crystallized medium are perfectly free from any restriction of any kind, are applicable to all kinds of substance, whether we suppose its structure to be analogous to that of a solid fluid or gas, and hold for all kinds of disarrangement, whether consisting of normal or transverse displacements, or both.

When we introduce the six relations between the constants above alluded to, and moreover assume that the vibrations constituting a polarized ray are *in* the plane of polarization, we arrive at Professor MacCullagh's equations*. If, on the contrary, we suppose the vibrations to be *perpendicular* to the plane of polarization, we arrive at equations which agree exactly with Fresnel's theory in every particular†.

If we introduce these six relations into the equations for crystallized media deduced from M. Cauchy's hypothesis, that the molecular forces act along the lines joining the different particles of the medium, it will be found that these equations are immediately reduced to the equations for an uncrystallized medium. From this it follows that M. Cauchy's hypothesis cannot be applied to any but uncrystallized media. In fact, it may be easily proved that if the equations derived from this hypothesis be true, a crystallized medium is incapable of propagating transverse vibrations.

Secondly, respecting the use of the symbolical method and notation above alluded to.

The application of the *symbolical method and notation* to the subject of vibratory motion is very remarkable, and leads to equations of great simplicity. In the case of an uncrystallized medium, the three

* Given in a paper read to the Royal Irish Academy, Dec. 9, 1839, page 14.

† On this subject see a paper by the late Mr. Greene in the seventh volume of the Cambridge Transactions, p. 121.

ordinary equations of motion are included in the single symbolication equation

$$\frac{d^2v}{dt^2}=B\left\{\frac{d^2}{dx^2}+\frac{d^2}{dy^2}+\frac{d^2}{dz^2}\right\}v+(A-B)\left(\alpha\frac{d}{dx}+\beta\frac{d}{dy}+\gamma\frac{d}{dz}\right)\left(\frac{d\xi}{dx}+\frac{d\eta}{dy}+\frac{d\zeta}{dz}\right).$$

If we employ the notation $\Delta u'.u$, and assume the symbol $\mathfrak{D}$ to represent the operation

$$\alpha\frac{d}{dx}+\beta\frac{d}{dy}+\gamma\frac{d}{dz},$$

the equation of motion becomes

$$\frac{d^2v}{dt^2}=B(\Delta\mathfrak{D}.\mathfrak{D})v+(A-B)+\Delta\mathfrak{D}.v\,;$$

or, by using the notation $Du'.u$ also, it may be put in the form

$$\frac{d^2v}{dt^2}=\{A\mathfrak{D}\Delta\mathfrak{D}.-B(D\mathfrak{D}.)^2\}v.$$

The symbol $\mathfrak{D}$ written before any quantity U which is a function of xyz, has a very remarkable signification; the *direction unit* of the symbol $\mathfrak{D}$U is that direction *perpendicular* to which there is no variation of U at the point xyz, and the *numerical magnitude* of $\mathfrak{D}$U is the *rate of variation* of U, when we pass from point to point *in that direction.*

The symbols $\Delta\mathfrak{D}.v$ and $D\mathfrak{D}.v$ have also remarkable significations. $\Delta\mathfrak{D}.v$ is a numerical quantity representing the *degree of expansion*, or what is called the *rarefaction* of the medium at the point xyz. $D\mathfrak{D}.v$ represents, in magnitude, the degree of *lateral disarrangement* of the medium at the point xyz, and, in direction, the *axis* about which that displacement takes place.

These two symbols may be found separately by the integration of an equation of the form

$$\frac{d^2U}{dt^2}=C\left(\frac{d^2U}{dx^2}+\frac{d^2U}{dy^2}+\frac{d^2U}{dz^2}\right).$$

When the six conditions above alluded to are introduced, the equation of motion for a crystallized medium becomes

$$\begin{aligned}\frac{d^2v}{dt^2}=&\left(A_1\alpha\frac{d}{dx}+A_2\beta\frac{d}{dy}+A_3\gamma\frac{d}{dz}\right)\Delta\mathfrak{D}.v\\&+D\mathfrak{D}.\left\{\left(B_2\frac{d\eta}{dz}-B'_3\frac{d\zeta}{dy}\right)\alpha+\left(B_3\frac{d\zeta}{dx}-B'_1\frac{d\xi}{dz}\right)\beta+\right.\\&\left.\left(B_1\frac{d\xi}{dy}-B'_2\frac{d\eta}{dx}\right)\gamma\right\},\end{aligned}$$

where $A_1\,A_2\,A_3$ are the three coefficients of *direct* elasticity with reference to the three axes of symmetry, and $B_1\,B_1'\,B_2\,B_2'\,B_3\,B_3'$ the six coefficients of *lateral* elasticity with reference to the same axes.

If the vibrations be transverse, this equation is reducible to the form

$$\begin{aligned}\frac{d^2v}{dt^2}&=-(D\mathfrak{D}.)^2(a^2\xi\alpha+b^2\eta\beta+c^2\zeta\gamma)\\&=-(D\mathfrak{D}.)^2(a^2\alpha\Delta\alpha+b^2\beta\Delta\beta+c^2\gamma\Delta\gamma)v,\end{aligned}$$

assuming the vibrations of a polarized ray to be *perpendicular* to the plane of polarization.

The well-known condition that a plane polarized ray may be transmissible without subdivision, and the velocity of propagation may be immediately deduced from this equation.

If we assume the vibrations of a polarized ray to be *in* the plane of polarization, the equation becomes

$$\frac{d^2 v}{dt^2} = -\mathrm{D}\mathfrak{D}.(a^2\alpha\Delta\alpha + b^2\beta\Delta\beta + c^2\gamma\Delta\gamma)\mathrm{D}\mathfrak{D}\, v.$$

This includes Professor MacCullagh's three equations.

PROCEEDINGS

OF THE

CAMBRIDGE PHILOSOPHICAL SOCIETY.

December 6, 1847.

On the Critical Values of the sums of Periodic Series. By G. G. Stokes, M.A., Fellow of Pembroke College, Cambridge.

There are a great many problems in heat, fluid motion, &c., the solution of which requires the development of an arbitrary function of x, $f(x)$, between certain limits as o and a of x, by means of functions of known form. The form of the expansion is determined, at least in part, by the conditions to be satisfied at the limits; and it is usually considered that these conditions are satisfied by adopting the form of expansion to which they lead. Thus, if the problem requires that $f(o)$ and $f(a)$ vanish, it is considered that this condition is satisfied by developing $f(x)$ in a series of sines of $\frac{\pi x}{a}$ and its multiples. But since an arbitrary function admits of expansion in such a series, the expanded function is not restricted to vanish at the limits o and a. It becomes then a question, how shall we know when the expanded function does really vanish at the limits, and if it does not, how are such expansions to be treated, and are they of any practical importance?

In considering the logic of such developments, the author was led to perceive in what manner the evanescence of $f(x)$ at the limits can be ascertained, or else the values of $f(o)$ and $f(a)$ obtained, from the development itself, even when the series cannot be summed, by examining the coefficient of $\sin\frac{n\pi x}{a}$ in the nth term. In a similar manner the discontinuity of $f(x)$ or any of its derivatives may be ascertained, and the amount of the sudden change of the function determined. In such cases the expansions of the derivatives of $f(x)$ cannot be obtained by differentiating under the sign of summation, but are given by formulæ which the author has considered.

The most important case in considering a series of sines, is that in which $f(x)$ is continuous; but $f(o)$ and $f(a)$, instead of being equal to zero, are given quantities, and the coefficients in the expansion are indeterminate. In this case the coefficients in the expansions of $f'(x)$ and $f''(x)$ contain, in addition to the indeterminate coefficients which enter into the expansion of $f(x)$, the given quantities $f(o)$ and

$f(a)$. Thus the expansion in a series of sines is useful, not only when $f(o)$ and $f(a)$ vanish, but also when they are given quantities. In the same way the expansion of $f(x)$ in a series of cosines is useful when $f'(o)$ and $f'(a)$ are given, as well as when they vanish. Thus, to take an example, the permanent temperature in a rectangular parallelepiped, when the temperatures of the faces are any arbitrary functions of the co-ordinates, can be expressed in a double series of sines involving *any two* of the three co-ordinates.

The author has only considered a series of sines and a series of cosines, with the corresponding integrals; but the methods which he has employed are of very general application. The comparison of different expressions of the same function of two or more independent variables often leads to very remarkable formulæ. The development of arbitrary functions in the way considered by the author is, however, not only curious but useful; for the expressions thus obtained are often much better adapted to numerical computation than those which would be obtained by the developments usually employed.

In connexion with these investigations, the author was led to consider the discontinuity of the sums of infinite series, or of the values of integrals between infinite limits, which sometimes takes place even when the series or integral remains convergent, and the general term of the series, or the quantity under the integral sine, is a continuous function of some quantity which is regarded as variable. The author has shown that in all such cases the convergency of the series or integral becomes infinitely slow.

The problem of determining the potential due to a given electrical point within a hollow conducting rectangular parallelepiped, and to the electricity included on the surface, is solved by a method which leads very readily to the result. The author thinks that a similar method may sometimes be advantageously employed in other questions. The electricity is first supposed to be diffused over a finite space: this allows of the expansion of the potential V in a triple series of sines. Instead of the equation $\nabla V=0$, where ∇ means the same as

$$\frac{d^2}{dx^2}+\frac{d^2}{dy^2}+\frac{d^2}{dz^2},$$

it becomes of course necessary to employ the equation $\nabla V=-4\pi\rho$. The solution having been obtained, the electricity may now be supposed to be condensed into a point, and one of the summations may be effected. The potential is thus expressed in a double series, which appears to be the simplest form that it admits of.

May 8, 1848.

Supplement to a paper "On the Intensity of Light in the neighbourhood of a Caustic." By G. B. Airy, Esq., Astronomer Royal.

The author, after referring to the paper printed in a former volume of the Society's Memoirs, in which he has shown that the expression for the intensity depends on the integral $\int_w \cos(w^3 - mw)$ between the limits $w=0$, $w =$ infinity, where m is proportional to the distance of the point at which the intensity is required, from the geometrical caustic, and in which he has calculated by quadratures the value of the definite integral for different values of m as far as $m = \pm 4{\cdot}0$, states that he was induced to have recourse to the method of quadratures only because every expansion which he attempted made it necessary to rely (for some of the terms) upon definite integrals equivalent to the integral $\int_\theta \cos\theta$ from $\theta=0$ to $\theta =$ infinity, and that he was not satisfied with the reasoning upon which some mathematicians had given a determinate value to that integral. Professor De Morgan, however, who felt no doubts upon it, had furnished him with a series proceeding by ascending powers of m, and had also explained in detail (in a letter embodied in this paper) his views on the evidence for the value of the series, and on the method of determining it. From this series, the values of the definite integral are computed for all the values of m for which the computation had been made by quadratures, and the result is that the two sets of computed numbers are entirely accordant. The computations are also extended to the limit $m = \pm 5{\cdot}6$, which is the greatest value to which it is possible to extend the calculations by the use of 10-figure logarithms.

May 22, 1848.

Some Remarks on the Theory of Matter. By R. L. Ellis, Esq., M.A., Fellow of Trinity College, Cambridge.

The question to which these remarks principally relate is this: Can all phænomena, *e. g.* those of chemistry, be explained mechanically? The writer, assuming that this question is to be answered negatively, endeavours to determine what principles of causation, beside mechanical force, may be introduced into physical theories, consistently with the doctrine that the secondary qualities of bodies are to be explained by means of the primary. His conclusion is, that we are at liberty, in constructing an hypothesis as to the mode of action of matter on matter, to introduce a new principle of causation (which he calls $(\text{force})^2$), bearing the same relation to force that force does to velocity; and further, that following the analogy here suggested, we may introduce an indefinite number of such principles, viz. $(\text{force})^3 \ldots (\text{force})^n$, &c., all essentially distinct from one another, and from those previously recognised.

But, on the other hand, he conceives that it is necessary to reject any modification of *qualitative* action; and that consequently physical science, though it may cease to be wholly *mechanical*, will yet always

continue to be *cinematical*, in the largest sense of which this word (so far as relates to local motion) can admit.

June 5, 1848.

Methods of Integrating Partial Differential Equations. By Prof. De Morgan. .

This paper contains a sketch of two distinct methods. In the first (x, y, z, p, q, r, s, t, having their usual significations) the given equation is supposed to be of the form $\phi(x, y, p, q)=0$, and this is made the result of elimination between two equations involving a new variable v. From these two, and their four differentials of the first order, p, q, r, s, t are eliminated, and an equation of the first order results between x, y, v. This last equation is often more manageable than the original one.

The process is rendered very simple when the given equation can be reduced to depend on two of the form

$$p=\phi(x, y, v) \quad q=\psi(x, y, v).$$

The second method was completed, Mr. De Morgan states, and out of his hands for transmission to the Society, when he discovered that Mouge had communicated it to the Institute, by which body it was never published. But M. Chasles found it among the manuscripts of the Institute, and stated it a few years ago in one of the notes to his *Aperçu Historique .... des Méthodes en Géométrie*. Its occurrence in the voluminous additions made to a work which itself treats only of geometry, seems to have prevented it from becoming known to any writer on the differential calculus. Certain particular cases appear in the writings of Legendre and Lacroix.

Let the equation be $\phi(x, y, z, p, q, r, s, t)=0$. Change x into p, y into q, z into $px+qy-z$, p into x, q into y, r into $\frac{t}{rt-s^2}$, s into $\frac{-s}{rt-s^2}$, t into $\frac{r}{rt-s^2}$. If the equation thus resulting can be integrated, let its solution be $Z=\psi(X, Y)$. Then the solution of the original equation can be obtained by eliminating X, Y, Z from

$$Z=\psi(X, Y) \quad x=\frac{dZ}{dX} \quad y=\frac{dZ}{dY} \quad z=xX+yY-Z.$$

In both methods the most effective mode of proceeding is to find what Lagrange calls a *primary solution*, containing two arbitrary constants, and then to use that primary solution.

On some new Fossil Fish of the Carboniferous Period. By Frederic M'Coy, M.G.S., N.H.S.D.

The author having premised that the species of fish of the carboniferous limestone enumerated in the third volume of the *Poissons Fossiles* of M. Agassiz are for the most part still unpublished, being without definitions or figures, states that through the kindness of

Capt. Jones, R.N., M.P., &c. he was enabled to study the original specimens of twenty-eight out of the thirty unpublished species from Armagh in M. Agassiz's list, and is therefore certain of the species described by him being so far distinct from those alluded to. The greater number of the examples here described are in the cabinets of the University of Cambridge (principally collected by the Rev. W. Stokes, of Caius College), and of Captain Jones; a few from the lower carboniferous shales of Ireland are only known in that of Mr. Griffith of Dublin. The descriptions are accompanied by drawings of all the species of the natural size, and illustrations of the microscopic structures; and acknowledgements are made of the kind co-operation of the Rev. Prof. Clark and Mr. Anthony of Caius College, Cambridge, in this part of the investigation, by allowing the use of their large microscopes, and assisting to prepare the transparent fragments for examination.

Twelve new genera are proposed:—1st. *Isodus*, for a fish of the yellow sandstone, having very numerous teeth with a simple conical pulp-cavity in their upper part, which divides into branches below as in *Rhizodus* (Owen); but the section is circular, and the teeth are all equal in size. 2nd. *Centrodus*, for curved conical teeth with a wide simple pulp-cavity, reducing the base to a sharp edge, and having not only the form but the microscopic structure of a reptile tooth, that is, from the simple pulp-cavity minute calcigerous tubes radiate to the circumference, terminating near the surface in a layer of small calcigerous cells, covered by a layer of true glass-like enamel, presenting no trace of structure with a power of 300 diameters, and quite distinct from that dense modification of *dentine*, which, forming the polished surface of most fish-teeth, has been confounded with true enamel, but which it is here proposed to call *ganoine* in future descriptions. 3rd. *Colonodus*, for very long simple teeth with simple pulp-cavity, and their sides indented by transverse wrinkles. 4th. *Osteoplax*, for large, flat, polygonal dermal plates, minutely wrinkled on the surface, and allied to *Psammosteus* (Ag.) of the Old Red Sandstone; but while the latter plates are composed of horizontal layers of large cells, the present genus has a very singular microscopic structure, being traversed by vertical branched (Haversian?) canals terminating in the pores of the surface; and in the intervening blastema are numerous oval Purkinian cells, the radiating tubuli of which do not anastomose. 5th. *Erismacanthus*, for a singular Ichthyodorulite not uncommon in the Armagh limestone, which, arising from a large compressed base, branches into two portions, one long anterior closely tuberculated prop-like portion, and another extruding backwards, short, and resembling a small *Ctenacanthus*, but with smooth ridges. 6th. *Platycanthus*, for small spines, extremely wide and compressed, resembling small Oracanths, but arched and with posterior rows of teeth. 7th. *Dipriacanthus*, for small, curved dorsal spines, which have two rows of denticles pointing downwards on the posterior face, and two rows pointing upwards on the anterior face, reminding us of the recent *Pimelodus* and *Synodontus* of the Nile. 8th. *Polyrhizodus*, an extraordinary genus of Psammodontoid teeth

not uncommon in the Armagh limestone, having the root divided into numerous fang-like lobes, as in a mammalian tooth. 9th. *Glossodus*, for certain tongue-shaped teeth allied to *Helodus* (Ag.). 10th. *Climaxodus*, for some palates allied to *Pœcilodus* (Ag.), but instead of being transversely trigonal and obliquely ridged, they are equilateral, and have the ridges transverse and parallel (like a flight of steps). 11th. *Chirodus*, for little hand-shaped teeth allied to the *Ceratodi*, but distinguished by the thumb-like lobe projecting from the middle of the long side, and which would prevent the union of the teeth in pairs in the mouth, in the manner of *Ceratodus*. 12th. *Petrodus*, small conical ridged teeth resembling limpets, common in the Derbyshire limestone, but presenting, of all known fossil fish, the nearest approach to the microscopic structure of the recent *Cestracion*. It is also proposed to divide the genus *Holoptychius* of M. Agassiz; and instead of considering it and *Rhizodus* of Owen as synonymous, to limit the latter to those great teeth with an elliptical section so common in some parts of the Carboniferous series, accompanied by large, thin, quadrate scales, marked with concentric lines of growth, and having a fine cancellated structure internally, the *Holoptychius Hibberti* (Ag.) *Rhizodus ferox*, (Owen) and *H. Portlocki* (Ag.) being the types; thus retaining the name *Holoptychius* for those fish so abundant in the Old Red Sandstone with thick, bony, ovate, longitudinally wrinkled scales, and minute teeth with a circular section, having the *H. nobilissimus*, *H. giganteus*, &c. as the type.

The number of new species described and figured in this paper is forty-one, of which several belong to genera not previously known in rocks of the carboniferous period, many showing a strong affinity to the Devonian type of form. Thus we have two species of *Psammosteus*, one of *Chelyophorus*, one (doubtful) of *Coccosteus*, one of *Asterolepis*, two of *Homacanthus*, and one of *Cosmacanthus*, genera hitherto only found in the Old Red Sandstone.

On an Absolute Thermometric Scale founded on Carnot's Theory of the Motive Power of Heat*, and calculated from Regnault's observations†. By Prof. W. Thomson, Fellow of St. Peter's College.

The determination of temperature has long been recognized as a problem of the greatest importance in physical science. It has accordingly been made a subject of most careful attention, and, especially in late years, of very elaborate and refined experimental re-

* Published in 1824 in a work entitled *Réflexions sur la Puissance Motrice du Feu*, by M. S. Carnot. Having never met with the original work, it is only through a paper by M. Clapeyron, on the same subject, published in the *Journal de l'Ecole Polytechnique*, vol. xiv. 1834, and translated in the first volume of Taylor's Scientific Memoirs, that the author has become acquainted with Carnot's theory.—W. T.

† An account of the first part of a series of researches undertaken by M. Regnault by order of the French Government, for ascertaining the various physical data of importance in the Theory of the Steam-Engine, is just published in the *Mémoires de l'Institut*, of which it constitutes the twenty-first volume (1847). The second part of the researches has not yet been published.

searches*; and we are thus at present in possession of as complete a practical solution of the problem as can be desired, even for the most accurate investigations. The theory of thermometry is however as yet far from being in so satisfactory a state. The principle to be followed in constructing a thermometric scale might at first sight seem to be obvious, as it might appear that a perfect thermometer would indicate equal additions of heat, as corresponding to equal elevations of temperature, estimated by the numbered divisions of its scale. It is however now recognized (from the variations in the specific heats of bodies) as an experimentally demonstrated fact that thermometry under this condition is impossible, and we are left without any principle on which to found an absolute thermometric scale.

Next in importance to the primary establishment of an absolute scale, independently of the properties of any particular kind of matter, is the fixing upon an arbitrary system of thermometry, according to which results of observations made by different experimenters, in various positions and circumstances, may be exactly compared. This object is very fully attained by means of thermometers constructed and graduated according to the clearly defined methods adopted by the best instrument-makers of the present day, when the rigorous experimental processes which have been indicated, especially by Regnault, for interpreting their indications in a comparable way, are followed. The particular kind of thermometer which is least liable to uncertain variations of any kind is that founded on the expansion of air, and this is therefore generally adopted as the standard for the comparison of thermometers of all constructions. Hence the scale which is at present employed for estimating temperature is that of the air-thermometer; and in accurate researches care is always taken to reduce to this scale the indications of the instrument actually used, whatever may be its specific construction and graduation.

The principle according to which the scale of the air-thermometer is graduated, is simply that equal absolute expansions of the mass of air or gas in the instrument, under a constant pressure, shall indicate equal differences of the numbers on the scale; the length of a "degree" being determined by allowing a given number for the interval between the freezing- and the boiling-points. Now it is found by Regnault that various thermometers, constructed with air under different pressures, or with different gases, give indications which coincide so closely, that, unless when certain gases, such as sulphurous acid, which approach the physical condition of vapours at saturation, are made use of, the variations are inappreciable†. This remarkable circumstance enhances very much the practical value of the air-thermometer; but still a rigorous standard can only be defined by

* A very important section of Regnault's work is devoted to this object.

† Regnault, *Relation des Expériences*, &c., Fourth Memoir, First Part. The differences, it is remarked by Regnault, would be much more sensible if the graduation were effected on the supposition that the coefficients of expansion of the different gases are equal, instead of being founded on the principle laid down in the text, according to which the freezing- and boiling-points are experimentally determined for each thermometer.

fixing upon a certain gas at a determinate pressure, as the thermometric substance. Although we have thus a strict principle for constructing a *definite* system for the estimation of temperature, yet as reference is essentially made to a specific body as the standard thermometric substance, we cannot consider that we have arrived at an *absolute* scale, and we can only regard, in strictness, the scale actually adopted as *an arbitrary series of numbered points of reference sufficiently close for the requirements of practical thermometry*.

In the present state of physical science, therefore, a question of extreme interest arises: *Is there any principle on which an absolute thermometric scale can be founded?* It appears to me that Carnot's theory of the motive power of heat enables us to give an affirmative answer.

The relation between motive power and heat, as established by Carnot, is such that *quantities of heat*, and *intervals of temperature*, are involved as the sole elements in the expression for the amount of mechanical effect to be obtained through the agency of heat; and since we have, independently, a definite system for the measurement of quantities of heat, we are thus furnished with a measure for intervals according to which absolute differences of temperature may be estimated. To make this intelligible, a few words in explanation of Carnot's theory must be given; but for a full account of this most valuable contribution to physical science, the reader is referred to either of the works mentioned above (the original treatise by Carnot, and Clapeyron's paper on the same subject).

In the present state of science no operation is known by which heat can be absorbed, without either elevating the temperature of matter, or becoming latent and producing some alteration in the physical condition of the body into which it is absorbed; and the conversion of heat (or *caloric*) into mechanical effect is probably impossible*, certainly undiscovered. In actual engines for obtaining mechanical effect through the agency of heat, we must consequently look for the source of power, not in any absorption and conversion, but merely in a transmission of heat. Now Carnot, starting from universally acknowledged physical principles, demonstrates that it is by the *letting down* of heat from a hot body to a cold body, through the medium of an engine (a steam-engine, or an air-engine for instance), that mechanical effect is to be obtained; and conversely, he proves that the same amount of heat may, by the expenditure of an equal amount of labouring force, be *raised* from the cold to the hot body (the engine being in this case *worked backwards*); just as mechanical

* This opinion seems to be nearly universally held by those who have written on the subject. A contrary opinion however has been advocated by Mr. Joule of Manchester; some very remarkable discoveries which he has made with reference to the *generation* of heat by the friction of fluids in motion, and some known experiments with magneto-electric machines, seeming to indicate an actual conversion of mechanical effect into caloric. No experiment however is adduced in which the converse operation is exhibited; but it must be confessed that as yet much is involved in mystery with reference to these fundamental questions of natural philosophy.

effect may be obtained by the descent of water let down by a water-wheel, and by spending labouring force in turning the wheel backwards, or in working a pump, water may be elevated to a higher level. The amount of mechanical effect to be obtained by the transmission of a given quantity of heat, through the medium of any kind of engine in which the œconomy is perfect, will depend, as Carnot demonstrates, not on the specific nature of the substance employed as the medium of transmission of heat in the engine, but solely on the interval between the temperatures of the two bodies between which the heat is transferred.

Carnot examines in detail the ideal construction of an air-engine and of a steam-engine, in which, besides the condition of perfect œconomy being satisfied, the machine is so arranged, that at the close of a complete operation the substance (air in one case and water in the other) employed is restored to precisely the same physical condition as at the commencement. He thus shows on what elements, capable of experimental determination, either with reference to air, or with reference to a liquid and its vapour, the absolute amount of mechanical effect due to the transmission of a unit of heat from a hot body to a cold body, through any given interval of the thermometric scale, may be ascertained. In M. Clapeyron's paper various experimental data, confessedly very imperfect, are brought forward, and the amounts of mechanical effect due to a unit of heat descending a degree of the air-thermometer, in various parts of the scale, are calculated from them, according to Carnot's expressions. The results so obtained indicate very decidedly, that what we may with much propriety call *the value of a degree* (estimated by the mechanical effect to be obtained from the descent of a unit of heat through it) of the air-thermometer depends on the part of the scale in which it is taken, being less for high than for low temperatures*.

The characteristic property of the scale which I now propose is, that all degrees have the same value; that is, that a unit of heat descending from a body A at the temperature T° of this scale, to a body B at the temperature $(T-1)^\circ$, would give out the same mechanical effect, whatever be the number T. This may justly be termed an absolute scale, since its characteristic is quite independent of the physical properties of any specific substance.

To compare this scale with that of the air-thermometer, the *values* (according to the principle of estimation stated above) of degrees of the air-thermometer must be known. Now an expression, obtained by Carnot from the consideration of his ideal steam-engine, enables

* This is what we might anticipate, when we reflect that infinite cold must correspond to a finite number of degrees of the air-thermometer below zero; since, if we push the strict principle of graduation, stated above, sufficiently far, we should arrive at a point corresponding to the volume of air being reduced to nothing, which would be marked as -273° ($-\frac{100}{\cdot 366}$, if $\cdot 366$ be the coefficient of expansion) of the scale; and therefore -273° of the air-thermometer is a point which cannot be reached at any finite temperature, however low.

us to calculate these values, when the latent heat of a given volume and the pressure of saturated vapour at any temperature are experimentally determined. The determination of these elements is the principal object of Regnault's great work, already referred to, but at present his researches are not complete. In the first part, which alone has been as yet published, the latent heats of a given *weight*, and the pressures of saturated vapour at all temperatures between 0° and 230° (Cent. of the air-thermometer), have been ascertained; but it would be necessary in addition to know the densities of saturated vapour at different temperatures, to enable us to determine the latent heat of a given *volume* at any temperature. M. Regnault announces his intention of instituting researches for this object; but till the results are made known, we have no way of completing the data necessary for the present problem, except by estimating the density of saturated vapour at any temperature (the corresponding pressure being known by Regnault's researches already published) according to the approximate laws of compressibility and expansion (the laws of Mariotte and Gay-Lussac, or Boyle and Dalton). Within the limits of natural temperature in ordinary climates, the density of saturated vapour is actually found by Regnault (*Etudes Hygrométriques* in the *Annales de Chimie*) to verify very closely these laws; and we have reason to believe from experiments which have been made by Gay-Lussac and others, that as high as the temperature 100° there can be no considerable deviation; but our estimate of the density of saturated vapour, founded on these laws, may be very erroneous at such high temperatures as 230°. Hence a completely satisfactory calculation of the proposed scale cannot be made till after the additional experimental data shall have been obtained; but with the data which we actually possess, we may make an approximate comparison of the new scale with that of the air-thermometer, which at least between 0° and 100° will be tolerably satisfactory.

The labour of performing the necessary calculations for effecting a comparison of the proposed scale with that of the air-thermometer, between the limits 0° and 230° of the latter, has been kindly undertaken by Mr. William Steele, lately of Glasgow College, now of St. Peter's College, Cambridge. His results in tabulated forms were laid before the Society, with a diagram, in which the comparison between the two scales is represented graphically.

In the first table, the amounts of mechanical effect due to the descent of a unit of heat through the successive degrees of the air-thermometer are exhibited. The unit of heat adopted is the quantity necessary to elevate the temperature of a kilogramme of water from 0° to 1° of the air-thermometer; and the unit of mechanical effect is a metre-kilogramme; that is, a kilogramme raised a metre high.

In the second table, the temperatures according to the proposed scale, which correspond to the different degrees of the air-thermometer from 0° to 230°, are exhibited. [The arbitrary points which coincide on the two scales are 0° and 100°.]

Note.—If we add together the first hundred numbers given in the

first table, we find 135·7 for the amount of work due to a unit of heat descending from a body A at 100° to B at 0°. Now 79 such units of heat would, according to Dr. Black (his result being very slightly corrected by Regnault), melt a kilogramme of ice. Hence if the heat necessary to melt a pound of ice be now taken as unity, and if a *metre-pound* be taken as the unit of mechanical effect, the amount of work to be obtained by the descent of a unit of heat from 100° to 0° is $79 \times 135{\cdot}7$, or 10,700 nearly. This is the same as 35,100 foot-pounds, which is a little more than the work of a one-horse-power engine (33,000 foot-pounds) in a minute; and consequently, if we had a steam-engine working with perfect œconomy at one-horse-power, the boiler being at the temperature 100°, and the condenser kept at 0° by a constant supply of ice, rather less than a pound of ice would be melted in a minute.

PROCEEDINGS

OF THE

CAMBRIDGE PHILOSOPHICAL SOCIETY.

May 17, 1847.

A Theory of the Transmission of Light through Transparent Media, and of Double Refraction, on the Hypothesis of Undulations. By Professor Challis.

The object of the author in this, as in two preceding communications on *Luminous Rays* and on the *Polarization of Light* *, is, to establish the undulatory theory of light on hydrodynamical principles, by means of a system of ray-vibrations, the motions in which are mathematically deduced from hydrodynamical equations. In applying these views to the transmission of light through transparent media, it is assumed that the æther is of the same uniform density and elasticity within any transparent medium as without; and that the diminished rate of propagation in the medium is owing to the obstacle which its atoms oppose to the free motion of the æthereal particles. Considering the proximity of the atoms to each other, and that the retarding effect of each atom at a given instant extends through many multiples of its linear dimensions, it is presumed that the mean retardation, though resulting from the presence of discrete atoms, may be regarded as continuous. It is also supposed that the mean effect of the presence of the atoms is to produce an *apparent* diminution of the elasticity of the æther, the motion in all other respects being the same as in free space. By the application of these principles, it is first shown that the *surface of elasticity*, that is, the surface whose radius vector drawn in any given direction represents the elasticity in that direction, is in general an *ellipsoid*. This being ascertained, the velocity of a *ray* in any given direction is investigated; and the result is, that the surface whose radius vectors drawn in any given direction represent the velocities of propagation of two oppositely polarized rays in that direction, is precisely the wave-surface in Fresnel's theory of double refraction.

March 6, 1848.

A Mathematical Theory of Luminous Vibrations. By Professor Challis.

This paper is intended to be supplementary to three former com-

* See Lond. Ed. and Dubl. Phil. Mag. vol. xxx. p. 365.

munications in which the undulatory theory is treated on hydrodynamical principles, and to elucidate or confirm results previously arrived at. In particular the author enters more at length into the mathematical theory of ray-vibrations, which, according to his views, correspond to rays of light. The principal theoretical deductions are,—(1.) that the longitudinal vibrations of a ray are defined by a function of the form $\sin \frac{2\pi}{\lambda}\left(z - a\,t\sqrt{1+\frac{e\lambda^2}{\pi^2}}\right)$, λ being the breadth of the undulation, and a, e certain constants; (2.) that light from any source is in general *composed* of rays for which a and $\frac{e\lambda^2}{\pi^2}$ are the same and λ different; (3.) that light coming immediately from its origin is *common* light, whatever be the nature of the cause producing it, and that to become *polarized* light, it must be acted upon by reflexion, refraction, &c.; (4.) that light coming immediately from its origin is seen in all directions.

November 13, 1848.

Second Memoir on the Fundamental Antithesis of Philosophy. By W. Whewell, D.D.

This memoir is a continuation of a former one in which the antithesis of thoughts and things, of ideas and facts, of subjective and objective, were shown to be at bottom the same antithesis, and to be a fundamental antithesis, the union of the two elements entering into all knowledge, and their separation being the test of all philosophy. The present memoir is employed in illustrating the proposition that the progress of science consists in the transfer of some truth from the factorial to the ideal side of the antithesis, or as it may be termed, in the *idealization of facts*. This is exemplified in mechanics, astronomy, botany and chemistry.

In a note, the author remarks on certain German systems of philosophy with reference to this antithesis. The Sensatorial school having reduced all knowledge to facts, Kant re-established the necessity of Ideas, which Fichte made almost the exclusive element. Schelling founded his philosophy upon the *absolute*, from which he derives both facts and ideas, but which a wiser philosophy shows us that we can never reach; and Hegel took the same foundation, but in a certain degree rightly pointed out that the progress towards the identity of fact and idea is to be traced in the history of science; which view, however, he has carried into detail by rash and blind conjecture.

On the Elements of Plane Geometrical Trigonometry, applicable to Trigonometrical Formulæ. By the Rev. F. Calvert.

The object of this paper is to define as distinctly as possible the elementary terms of trigonometry, and to explain the conventional use of the negative sign in expressing such simple functions of angles as the sine, cosine, tangent, &c.

November 27, 1848.

On a Difficulty suggested by Professor Challis in the Theory of Sound. By Robert Moon.

In a paper by Professor Challis contained in the Supplementary Number of the 32nd Volume of the Philosophical Magazine, I find the following:—

"The difficulty respecting the augmentation of the velocity of sound by the development of heat, cannot be so summarily disposed of as Mr. Airy appears to imagine. I shall perhaps succeed better in conveying my meaning by using symbols. If θ be the temperature where the pressure is p and density ρ, and θ_1 the temperature in the quiescent state of the fluid, we have, by a known equation,

$$p=a^2\rho(1+\alpha.\overline{\theta-\theta_1}).$$

Hence

$$\frac{d^2z}{dt^2}=-\frac{dp}{\rho dz}=-\frac{a^2d\rho}{\rho dz}-a^2\alpha(\theta-\theta_1)\frac{d\rho}{\rho dz}-a^2\alpha\frac{d\theta}{dz}. \qquad (1.)$$

"The usual theory explains how the third term of the right-hand side of this equation may be in a given ratio to the first; but my difficulty is to conceive how the same can be the case also with the *second* term, since it changes sign with the change of sign of $\theta-\theta_1$."

I conceive that the explanation, according to the usual theory to which Professor Challis here alludes, depends upon the principle, "that for very small condensations of air, the rise of temperature will be proportional to the increase of density." (Vide Herschel On Sound, *Encyc. Met.*, art. 72.) Thus we may put

$$\theta-\theta_1=k(1-\rho),$$

where k is a constant, and 1 is put for the density of equilibrium: on which hypothesis it is obvious that the third term of equation (1.) will be a multiple of the first, as described by Prof. Challis. It also follows that the second term vanishes, since it has $(1-\rho)$ for a factor, and in reducing (1.) to the ordinary form of the differential equation of sound the difference between ρ and 1 is neglected. It thus, I think, appears that the difficulty suggested by Prof. Challis has no real existence.

Observations of the Aurora Borealis of Nov. 17, 1848, made at the Cambridge Observatory. By Professor Challis.

These observations relate principally to the corona, or point of apparent convergence of the streamers, the remarkable display of Nov. 17 being peculiarly favourable for noting the position of this critical point. They were taken partly by estimation of distances from stars, and partly by a small altitude and azimuth instrument (called by the author a *meteoroscope*), which is furnished with a bar, eighteen inches long, carrying at one end a rectangular piece whose edges are horizontal and vertical, by looking at which through an eyelet-hole, about the size of the pupil of the eye, at the other end, the collimation is performed. Each observed position is compared

with the point of the heavens to which the south end of the dipping-needle was directed at the time of observation. This point was ascertained by means of observations of declination, horizontal force, and vertical force, taken at the Greenwich Observatory during the prevalence of the aurora by Mr. Brooke's photographic process, the results of which were communicated to the author by the Astronomer Royal. It is assumed that the magnetic declination and dip at Cambridge differ from those at Greenwich at any given time by certain constant quantities, whether the magnet be disturbed or not. These constant differences were derived from the following formulæ :—

$$V-V_0=0{\cdot}142518\lambda + 0{\cdot}159548l$$
$$D-D_0=0{\cdot}027713\lambda + 0{\cdot}513523l,$$

in which V and D are the declination and dip at a place not very distant from Greenwich, V_0 and D_0 the contemporaneous declination and dip at Greenwich, λ the longitude of the place *west*, in *seconds of time*, and l the *excess* of its latitude in *minutes* above that of Greenwich. These are merely formulæ of interpolation by simple differences derived from the following data :—

	Lat.	Long. West.	Declination in 1843.	Dip in 1843.
	° ′	m. s.	° ′	° ′
Greenwich	51 28·6	0 0·0	23 17·59	69 1·9
Makerstoun....	55 34·7	10 3·5	25 22·85	71 25·0
Dublin........	53 21·0	25 4·0	27 9·87	70 41·3

The above are very accurate contemporaneous values of the declination and dip at the three places, and the formulæ derived from them will probably apply with considerable accuracy to any place in the United Kingdom at any date not very remote from 1843. For the Cambridge observatory $V-V_0=+3'{\cdot}7$ and $D-D_0=+22'{\cdot}0$.

The mean result from 24 observations of the position of the corona is, that it was situated 5′ further from the astronomical zenith, and 1° 14′ nearer to the meridian than the point of the heavens to which the south end of the dipping-needle was directed.

The places of the corona given by the different observations exhibit considerable discrepancies, which are accounted for by saying, that as the formation of the corona is merely an effect of *perspective*, its position varies, since the streamers are not exactly parallel, with the locality from which they rise; also with any variation of their direction at a given locality; and, supposing the course of the streamers to be somewhat curved in their ascent, it will vary with the height to which they rise. Accordingly, as appeared to be the fact, the corona would be continually shifting its position within certain limits.

Prof. Challis has made a similar comparison with observations of the position of the corona of the same aurora made at Haverhill, at Darlington, and at Bath; also with observations at Whitehaven of the aurora of Oct. 18, 1848, and of that of Oct. 24, 1847, at Cam-

bridge. From a consideration of all the results derived from the discussion of observations made on different occasions and at different places, the following conclusions seem to be established:—

First, that the corona of an aurora borealis is formed near the point of the heavens to which the south end of the dipping-needle at the place of observation is directed.

Secondly, that the observations, while they indicate no decided difference of altitude between the two points, show with great probability that the corona is the more *westerly* by about $1\frac{1}{4}°$ measured on an arc perpendicular to the meridian.

The paper concludes with a particular description of the aurora borealis of Nov. 17 as observed at the Cambridge Observatory, and with three tables of the observations of declination, horizontal force, and vertical force, made at Greenwich, and used in the calculations. These observations present so striking an instance of great magnetic disturbances occurring simultaneously with an extraordinary display of the aurora borealis, that the connexion in some way of the two kinds of phænomena must be regarded as a physical fact.

On Clock Escapements. By E. B. Denison, Esq., of Trinity College.

The object of this paper is, first to point out the real cause of the general excellence of the dead beat escapement; and secondly, to show that in a gravity, or remontoir escapement, in which the pendulum raises an arm carrying a small weight, from an angle γ up to its extreme semiarc α, which follows the pendulum down again to an angle β (either $+$ and less than γ, or $= -\gamma$), there is a certain proportion between α, β, and γ, which will cause the errors of the clock for small variations of α to be much smaller than in the dead escapement, and in fact inappreciable.

The author adopts the equations obtained by Mr. Airy in his paper on this subject in vol. iii. of the Transactions of the Society, and shows that the increase of the time of an oscillation

$$= \Delta\left(\frac{d\phi}{\phi} - \frac{3d\alpha}{\alpha}\right),$$

where Δ is the difference between the time of oscillation of a free pendulum and one affected by this escapement (which in clocks of the best construction he shows will amount to about 1 second a day); ϕ is the angular accelerating force of the escapement on the pendulum; $d\phi$ the variation in this force due to the variation of the friction of the train and of the state of the oil on the acting part of the pallets; $d\alpha$ the variation of the arc from the same causes, and also from the state of the oil on the dead or circular part of the pallets. It appears therefore that the two causes of error have a tendency to correct each other; and in practice it is found that $\frac{3d\alpha}{\alpha}$ is generally not far short of $\frac{d\phi}{\phi}$, which is the reason of these clocks going so well.

In a gravity escapement there is no variation of the force, and

the author shows from Mr. Airy's equations that $\frac{d\Delta}{d\alpha}=0$ if $\alpha=\gamma\sqrt{2}$ in that escapement where the remontoir weight is taken up at γ and follows the pendulum again to $-\gamma$; and in the other kind of gravity escapement $\frac{d\Delta}{d\phi}\frac{d\Delta}{d\alpha}=0$ when

$$\alpha^2=2\sqrt{\alpha^2-\gamma^2}\sqrt{\alpha^2-\beta^2}.$$

This last construction however is barely practicable, if this condition is to be satisfied, on account of the small difference between β and γ which is allowed by the deduction of the value necessary for $\alpha-\gamma$, the angle in which the unlocking of the escapement is effected; although this is the construction which has been used in nearly all the gravity escapements that have been tried; and of course the proper condition has been very far from satisfied, and the clocks have failed.

In a supplement to this paper the author proposes, chiefly for turret clocks, a new construction of a spring remontoir on the axis of the escape-wheel. The object of such remontoirs is to remove from the escapement (of any ordinary kind) the great inequalities of force caused by the varying friction of the heavy train and dial-work, and by the action of the wind on the hands; and also to cause the minute-hand to move only at visible intervals, such as $\frac{1}{2}$ a minute, and the striking to take place exactly at the right second. The Royal Exchange clock, made under the superintendence of the Astronomer Royal, has a gravity remontoir in the train introduced for these purposes; but it is too complicated and expensive for ordinary use, and has a good deal of friction, from which the proposed remontoir is free. Spring remontoirs winding up at similar intervals have been tried in France, but without success, from defects in their construction.

December 11, 1848.

On the Formation of the Central Spot of Newton's Rings beyond the Critical Angle. By G. G. Stokes, M.A., Fellow of Pembroke College, Cambridge.

It has long been known that when Newton's rings are formed between the under surface of a prism and the upper surface of a lens, or of a second prism, so as to allow of increasing the angle of incidence at pleasure, the rings disappear when the critical angle is passed, but the central spot remains. The existence of the spot under these circumstances has even been attributed to the disturbance in the second medium, which, when the angle of incidence exceeds the critical angle, takes the place of that disturbance which at a smaller incidence constitutes the refracted light; but the expression for the intensity has not hitherto been given, so far as the author is aware. The object of the author in the present paper is to supply this deficiency.

The author has not adopted any particular dynamical theory, but has deduced his results from Fresnel's expressions for the intensities of reflected and refracted polarized light. When the angle of incidence becomes greater than the critical angle these expressions become imaginary. When the imaginary expressions are interpreted in the way in which physical considerations show that they must be interpreted, it becomes easy to obtain the expression for the intensity of the light, whether reflected or transmitted, in the neighbourhood of the spot. When the first and third media are of the same nature, the following expression is obtained for the intensity (I) of the reflected light, the incident light being polarized in the plane of incidence, and its intensity being taken for unity,

$$I = \frac{(1-q^2)^2}{(1-q^2)^2+4q^2\sin^2 2\theta}, \text{ where } q = \varepsilon^{-\frac{2\pi D}{\lambda}\sqrt{\mu^2 \sin^2 i - 1}}.$$

In this expression μ is the refractive index of the first medium, i the angle of incidence on the surface of the second medium, or interposed plate of air, D the thickness of that plate at the point considered, λ the length of a wave in air, 2θ the acceleration of phase due to total internal reflexion. When the light is polarized perpendicularly to the plane of incidence, it is only necessary to replace 2θ by 2ϕ, the angles θ, ϕ being those so denoted in Airy's Tract. The intensity of the transmitted light is obtained by subtracting that of the reflected light from unity.

From the expression for the intensity, the author has deduced the following results, all of which he has verified by observation.

The spot is comparatively large near the critical angle, and becomes smaller and smaller as the angle of incidence increases. Near the critical angle the fainter portion, or *ragged edge*, of the bright spot seen by transmission is broad; at considerable angles of incidence the light decreases with comparative abruptness. Towards the edge of the spot there is a predominance of the colours at the red end of the spectrum, causing the ragged edge to appear brown. Near the critical angle the spot is larger for light polarized perpendicularly to the plane of incidence than for light polarized in that plane: at considerable angles of incidence the order of magnitude is reversed. The difference is far more conspicuous in the former case than in the latter, and in that case consists principally in the greater extent of the ragged edge. When the incident light is polarized at an azimuth of 45°, or thereabouts, and the transmitted light is analysed so as to extinguish the light transmitted near the point of contact, there is seen a central dark patch surrounded by a luminous ring.

February 26, 1849.

On a New Method of finding the Rational Roots of Numerical Equations. By Robert Moon, Esq.

The author proposes to found a new experimental method of find-

ing the integral roots of numerical equations upon the following theorem.

If the equation

$$x^n+p_1x^{n-1}+p_2x^{n-2}+\text{ \&c. }+p_ix^{n-i}+\text{ \&c.}+p_n=0$$

has a positive and integral root m, we shall have $-p_n$ equal to m terms of the following series :—

$$A_{n-1}+1.\,A_{n-2}+1.2.\,A_{n-3}+1.2.3\,A_{n-4}+\text{ \&c. }+1.2\ldots(n-i+1)\,A_i$$
$$+\text{ \&c. }+1.2\ldots(n-1)\,A_0$$
$$+A_{n-1}+2.\,A_{n-2}+2.3\,A_{n-3}+2.3.4\,A_{n-4}+\text{ \&c. }+2.3\ldots(n-i+2)\,A_i$$
$$+\text{ \&c. }+2.3\ldots n\,A_0$$
$$+A_{n-1}+3.A_{n-2}+3.4\,A_{n-3}+3.4.5\,A_{n-4}+\text{ \&c. }+3.4\ldots(n-i+3)\,A_i$$
$$+\text{ \&c. }+3.4\ldots(n+1)\,A_0$$
$$+A_{n-1}+4.\,A_{n-2}+4.5\,A_{n-3}+4.5.6\,A_{n-4}+\text{ \&c. }+4.5\ldots(n-i+4)\,A_i$$
$$+\text{ \&c. }+4.5\ldots(n+2)\,A_0$$
$$\text{\&c.} \qquad\qquad \text{\&c.}$$

where

$$A_i=(n-i)(p_i-h_1p_{i-1}+h_2p_{i-2}+\text{\&c.}+(-1)^rh_rp_{i-r}+\text{\&c.}\pm h_{i-1}p_1\mp h_i)$$

and

h_1=the sum of the natural numbers 1, 2, 3..... $(n-i)$.

h_2=the sum of the homogeneous products of the same quantities of two dimensions.

h_3=the sum of the homogeneous products of the same quantities of three dimensions, and so forth.

From the above formula for A_i may be determined all the coefficients A except the first, which is determined from the equation

$$A_{n-1}=p_{n-1}-p_{n-2}+p_{n-3}-\text{\&c.}\pm p_1\mp 1.$$

Having determined the quantities A in any particular case, let them be substituted in the first line of the series. If the sum of that line be equal to $-p_n$ unity is a root of the equation. Let the second line be then written down and added to the first. If the sum of the two equals $-p_n$, 2 is a root of the equation, and so by adding successive lines we shall ascertain whether the successive integers 3.4., &c. are or are not roots of the equation.

The quantities h in the expression for A_i depend upon the number of the coefficient and the number of the dimensions of the equation. The author proposes that these should be calculated and tabulated for equations of all dimensions up to a certain limit, by which means we should be in possession of so many skeletons of equations, ready for application in any particular case, and the calculation in particular instances would be thus greatly facilitated.

It will be observed that each successive line is derived from that preceding by a simple division and multiplication of the separate terms of the latter, and thus each succeeding trial facilitates those which follow; contrary to what obtaind in the ordinary method by successive substitutions, in which each attempt proceeds *de novo*.

If the addition of a term makes the series from being greater than p_n less than it, or *vice versa*, a fractional or surd root will lie between

the number expressing the number of the term so added and the number next below it.

If all the roots are impossible, the series will be either always greater or always less than p_n, whatever be the number of terms taken.

For an example take the cubic

$$x^3+px^2+qx+r=0.$$

Here
$$\begin{aligned}-r=&3\times 1.2+2(p-3)1+q-p+1\\&+3\times 2.3+2(p-3)2+q-p+1\\&+3\times 3.4+2(p-3)3+q-p+1\\&+\ \&c.\ \text{to } x \text{ terms,}\end{aligned}$$

if x is a positive integer.

The method in common with other experimental methods applies to the discovery of all roots, possible or impossible, which do not involve surds.

March 12, 1849.

On the Intrinsic Equation to a Curve, and its application. By the Master of Trinity.

The author remarked that the expressions for the lengths of curves, their involutes and evolutes, in the ordinary methods, are complex and untractable, which arises in a great measure from the properties of *extrinsic* lines being introduced, namely, coordinates. But a curve may be represented without any such additions, by an equation between the length and the angle of flexure, which is therefore called the *intrinsic* equation. This equation gives, with remarkable facility, the radii of curvature; involutes and evolutes of most curves. It also expresses very simply what may be called *running* curves; namely, curves which run like a pattern along a strip of ornamented work. A very simple equation expresses, for instance, the inclined scroll pattern so common in the antique, and by altering the constants, gives to this pattern an endless variety of forms. If s be the length of the curve and ϕ the angle, the *intrinsic* equation to the circle is $s=a\phi$; to the cycloid $s=a\sin\phi$. The equation to an epicycloid or hypocycloid is $s=a\sin m\phi$, according as m is less or greater than unity. The equation to an undulating pattern is $\phi=m\sin s$, which assumes very various shapes by varying m. The method was also used in proving that if we take the successive involutes of a curve an indefinite number of times, the resulting curve (with certain limitations) tends to become *the equiangular spiral* if the unwrapping be always in the same direction, and tends to become *the cycloid* if the unwrapping be alternately in opposite directions. The latter proposition had already been discovered by Bernouilli and proved by Euler.

April 23, 1849.

On the Variation of Gravity at the Surface of the Earth. By G. G. Stokes, M.A., Fellow of Pembroke College, Cambridge.

In the theory of the figure of the earth on the hypothesis of original fluidity, a simple expression is obtained for the variation of gravity along the surface, which contains the numerical relation between the ellipticity and the ratio of polar to equatoreal gravity, known as Clairaut's theorem. The demonstration, however, of this expression does not require the hypothesis of original fluidity, if the spheroidal form of the surface and its perpendicularity to the direction of gravity be assumed as results of observation. On the hypothesis merely that the earth consists of nearly spherical strata of equal density, Laplace has established a connexion between the form of the surface, regarded as a surface of equilibrium, and the variation of gravity along it; and in the particular case in which the surface is an oblate spheroid of small ellipticity, having its axis of figure coincident with the axis of rotation, the expression which results for the variation of gravity is identical with that which is obtained on the hypothesis of original fluidity. The object of the author in the first part of this paper is to obtain the general connexion between the form of the surface and the variation of gravity along it, by an application of the doctrine of potentials, without making any hypothesis whatsoever respecting the distribution of matter in the interior of the earth.

The latter part of the paper was devoted to the consideration of the irregularities produced in the variation of gravity by the irregular distribution of land and sea at the surface of the earth. The author has shown why gravity should appear less on continents than on small islands situated at a distance from any continent, which is a circumstance that has long since been observed. The result is accounted for by the elevation of the sea-level produced by the attraction of a continent, in consequence of which a station on a continent is further removed from the centre of the earth than it appears to be. It is shown also that the numerical value of the earth's ellipticity, which has been deduced from pendulum experiments, is somewhat too great, in consequence of the undue proportion of oceanic stations in low latitudes, among the group of stations at which the observations were made which have been employed in the discussion.

The author has given formulæ whereby observed gravity may be corrected for the irregularities of the earth's surface. These formulæ require a knowledge, or at least an approximate knowledge, of the height of the land and the depth of the sea throughout the earth's surface. The sign and magnitude of the difference between observed gravity, and gravity calculated on the hypothesis of the earth's original fluidity, appears on the whole to depend on the insular or continental character of the station at which the observation has been taken. This circumstance renders it probable, that if observed gravity were corrected for the irregular attraction due to the irregular

distribution of sea and land throughout the whole surface of the earth, the result would agree far better with gravity calculated on the hypothesis of original fluidity.

May 7, 1849.

Additional Note to a Memoir on the Intrinsic Equation of Curves. By Dr. Whewell.

This note contained an extension of a theorem discovered by John Bernouilli, and demonstrated by Euler, to this effect: that if from any *rectangular* curve a string be unwrapped, and from the curve so described again a string unwrapped, and so on perpetually and alternately in opposite directions, the curves constantly tend to the form of the common cycloid. The extension is to this effect: that if the original curve be not rectangular, the curves perpetually tend to the form of an epicycloid or hypocycloid, according as the angle is greater or less than a right angle.

May 21, 1849.

Discussion of a Differential Equation relating to the breaking of Railway Bridges. By G. G. Stokes, M.A., Fellow of Pembroke College.

In August 1847 a Royal Commission was appointed "for the purpose of inquiring into the conditions to be observed by engineers in the application of iron in structures exposed to violent concussions and vibration." Among other branches of inquiry, the members of the Commission have lately been making experiments on the motion of a carriage, variously loaded in different experiments, which passed with different velocities over a slight iron bridge; the object of the experiments being to examine the effect of the velocity of a train in increasing or decreasing the tendency of a bridge over which the train is passing to break under its weight. The remarkable result was obtained, that the deflection is in some cases much greater than the central statical deflection, and that the greatest deflection takes place after the body has passed the centre of the bridge. In investigating the theory of the motion, reducing the problem to the utmost degree of simplicity by regarding the moving carriage as a heavy particle, and neglecting the inertia of the bridge, Professor Willis, who is a member of the Commission, was led to a differential equation of the form

$$\frac{d^2y}{dx^2}=a-\frac{by}{(2cx-x^2)^2},$$

where x, y are the horizontal and vertical co-ordinates of the moving body, $2c$ is the length of the bridge, and a, b are certain constants. Professor Willis requested the author's consideration of this equation, with a view to obtain numerical results, and to determine, if possible, the velocity which produces a maximum deflection.

The author has expressed y in a series according to ascending

powers of x, which is convergent when $x < 2c$. The convergency, however, becomes very slow when x approaches the limit $2c$; and the series does not point out the law according to which $f(x)$ or y approaches its extreme value 0 as x approaches $2c$. When the constant term in the second member of the preceding equation is omitted, the equation may be integrated in finite terms; and consequently the variables can be separated in the actual equation, so that $f(x)$ can be expressed explicitly by means of definite integrals. In this way the author has obtained $f(2c-x)-f(x)$ in finite terms, so that the numerical value of $f(x)$ may readily be obtained from $x=c$ to $x=2c$, after it has been calculated from the series from $x=0$ to $x=c$: and between these limits the series is very convergent, being ultimately a geometric series with a ratio $\frac{x}{2c}$. The author has also investigated a series proceeding according to ascending powers of $c-x$, which converges more rapidly than the former when x approaches c. By the use of these two series, $f(x)$ may be calculated by means of series which are ultimately geometric series, with ratios ranging from 0 to $\frac{1}{2}$.

The unsymmetrical form of the trajectory, and the largeness of the deflection produced by the moving body, come out from the investigation. By means of the numerical values of $f(x)$ the author has drawn a figure representing the trajectory for four different velocities. The expression for the central deflection, however, becomes infinite when x becomes equal to $2c$, which shows that it is necessary to take into account the inertia of the bridge; although, if the bridge be really light, the solution obtained when the inertia of the bridge is neglected may be sufficiently exact for the greater part of the body's course.

On Hegel's Criticism of Newton's *Principia*. By Dr. Whewell.

Parts of Hegel's *Encyclopædia* are here examined with the purpose of testing the value of his philosophy, not of defending Newton. Hegel says that the glory due to Kepler has been unjustly transferred to Newton; confounding thus the discovery of the laws with the discovery of the force from which the laws proceed, in which latter discovery Kepler had no share. Hegel pretends to derive the Newtonian "formula" from the Keplerian law, thus;—by Kepler's law, A being the distance, and T the periodic time, $\frac{A^3}{T^2}$ is constant: but Newton (Hegel says) *calls* $\frac{A}{T^2}$ universal gravitation, whence universal gravitation is inversely as A^2:—a most absurd misrepresentation of the course of Newton's reasoning. In the same manner Hegel criticises, and utterly misrepresents Newton's explanation, for the elliptical orbit, of the body's approaching to and receding from the centre; and of the reason why the body moves in an ellipse. Hegel also offers his own explanation of Kepler's laws from his own *à priori* assumptions. He says that the motion of the heavenly bodies is not a being pulled this way or that, as is imagined by the Newtonians; they go along, as the ancients said, like blessed gods.

PROCEEDINGS

OF THE

CAMBRIDGE PHILOSOPHICAL SOCIETY.

November 26, 1849.

On the Dynamical Theory of Diffraction. By Professor Stokes.

The problem of diffraction is treated mathematically by conceiving each wave of a series incident on a small aperture, or passing the edge of a diffracting body, broken up on arriving at the aperture or diffracting edge, regarding each element of the wave as the centre of an elementary disturbance, which diverges spherically from that element, and finding by integration the aggregate disturbance at any point in front of the primary wave. With the exception of one case of diffraction, which will be mentioned further on, the illumination in front of an aperture is insensible except in the immediate neighbourhood of a normal to the primary wave drawn through a point in the aperture. Consequently we are only concerned with the law of disturbance in that part of a secondary wave which lies very near the normal to the primary wave; the nature of the disturbance in other directions does not affect the result, since the secondary waves neutralize each other by interference. Now it has been shown by others, by indirect methods, that if c be the coefficient of vibration in the incident light, dS an element of the area of the aperture, r the radius of a secondary wave diverging from dS, λ the wave length, the coefficient of vibration in the secondary wave will be $\frac{c\,dS}{\lambda r}$, and the phase of vibration must be accelerated by a quarter of an undulation; or in other words, $\frac{\lambda}{4}$ must be subtracted from the retardation due to the radius r. These results, however, according to what has been already remarked, only apply to that portion of a secondary wave which lies immediately about the normal to the primary. The object of the author in this paper was to determine, on purely dynamical principles, the law of disturbance *in any direction* in a secondary wave.

The author has treated the æther as an elastic solid; and as such it must be treated in considering light, if the theory of transverse vibrations be not rejected. The object which he had in view required the solution, in the first instance, of the following problem;—to determine the disturbance at any point of an elastic medium, and

at any time, due to a given small arbitrary disturbance confined to a finite portion of the medium. This problem was solved long ago by Poisson; but the author has given a totally different solution of it, which appears to be in some respects simpler than Poisson's. In the course of the solution, the author was led incidentally to the following very general dynamical theorem.

Let any material system whatsoever, in which the forces acting depend only on the positions of the particles, be slightly disturbed from a position of equilibrium, and then left to itself: then the part of the disturbance at any time which depends on the initial displacements will be got from that which depends on the initial velocities by differentiating with respect to the time, and replacing the arbitrary functions, or arbitrary constants, which express the initial velocities by those which express the corresponding initial displacements. Particular cases of this theorem are of frequent occurrence, but the author is not aware of any writing in which the theorem is enunciated in all its generality.

The problem above-mentioned has been applied by the author to the case of diffraction in the following manner. Conceive a series of plane waves of plane-polarized light propagated in a direction perpendicular to a fixed mathematical plane P. According to the principle of the superposition of small motions, we have a perfect right to consider the disturbance of the æther in front of the plane P as the resultant of the elementary disturbances corresponding to the several elements of P. Let it be required to determine the disturbance which corresponds to an elementary portion only of the plane P. In this consists the whole of the dynamical part of the theory of diffraction, if we except the case of diffraction at the common surface of two different media; the rest is a mere question of integration. Let the time t be divided into equal intervals, each equal to τ. The disturbance which is propagated across the plane P during the first interval τ occupies a layer of the medium having a thickness $v\tau$, if v be the velocity of propagation, and consists of a certain velocity and a certain displacement. By the problem above mentioned, we can find by itself the effect of the disturbance which occupies so much only of this layer as corresponds to a given element dS of P. By doing the same for the 2nd, 3rd, &c. intervals τ, and then making the number of such intervals increase and their magnitude decrease indefinitely, we shall get the effect of the disturbance which is continually transmitted across dS. The result is a little complicated, but is much simplified when certain terms are neglected which are only sensible when the radius of the secondary wave is comparable with λ, and which are wholly insensible in the physical applications of the problem. The result thus simplified may be enunciated as below:—In the enunciation, the term *diffracted ray* is used to denote the disturbance in an elementary portion of a secondary wave, diverging in a given direction from the centre; the plane containing the incident and diffracted rays will be called the *plane of diffraction*, the supplement of the angle between these two rays the *angle of diffraction*, and the plane passing

through a ray of plane-polarized light and containing the direction of vibration the *plane of vibration*.

The incident ray being plane-polarized, each diffracted ray will be plane-polarized, and the plane of polarization will be determined by the following law:—*The plane of vibration of the diffracted ray is parallel to the direction of vibration of the incident ray.* The direction of vibration being thus determined, it remains only to specify its magnitude. Let

$$\zeta = c \sin \frac{2\pi}{\lambda}(vt - x)$$

be the displacement in the case of the incident light, ζ' the displacement in the case of the diffracted ray, ζ' being reckoned positive in the direction which makes an acute angle with that in which ζ is reckoned positive. Let r be the radius of the secondary wave diverging from dS, and let r make angles θ with the direction of propagation of the incident ray, and ϕ with the direction of vibration; then

$$\zeta' = \frac{cdS}{2\lambda r}(1 + \cos\theta)\sin\phi\cos\frac{2\pi}{\lambda}(vt - r) \quad . \quad . \quad . \quad \text{(a.)}$$

When an arbitrary function of $vt - x$, $f(vt - x)$ occurs in ζ, it is not $f(vt - r)$ but $f'(vt - r)$ that appears in ζ', where f' denotes the derivative of f, and accordingly in the particular case in which $f(u) = \sin u$ the sine in ζ is replaced in ζ' by a cosine. It may readily be verified, that if the formula (a.) be applied to determine by integration the disturbance which corresponds to the whole of the plane P, the disturbance in front is the same as if the wave had not been supposed broken up, and no disturbance is propagated backwards.

The law obtained for determining the position of the plane of polarization of the diffracted ray seems to lead to a crucial experiment for deciding between the two rival theories between the directions of vibration in plane-polarized light. Suppose the incident light polarized by transmission through a Nicol's prism mounted in a graduated instrument, and let the diffracted light be analysed in a similar manner. By means of the graduation of the polarizer, we can turn the plane of polarization of the incident ray, and consequently the plane of vibration, which is either parallel or perpendicular to the plane of polarization, round through equal angles of say 5° or 10° at a time. According to theory, the *planes of vibration* of the diffracted ray will not be distributed uniformly, but will be crowded towards the perpendicular to the plane of diffraction. But experiment will enable us to decide whether the *planes of polarization* are crowded towards the plane of diffraction or towards the perpendicular to the plane of diffraction, and we shall accordingly be led to conclude, either that the vibrations are perpendicular, or that they are parallel to the plane of polarization.

In ordinary cases of diffraction, the illumination, in consequence of interference, is insensible beyond a small angle of diffraction. It is only by means of a fine grating that we can procure light of considerable intensity that has been diffracted at a large angle. The

author has been enabled to perform the experiment, or rather a modification of it, by the kindness of his friends Professors Miller and O'Brien; of whom the former lent him a fine glass-grating, consisting of a glass plate on which parallel and equidistant lines had been ruled with a diamond at the rate of 1300 to the inch, and the latter lent him the graduated instruments required. The theory does not quite meet the case of a glass-grating, in which the diffraction takes place at the common surface of two media, but it leads to a definite result on each of the two extreme suppositions:—1st, that the diffraction takes place before the light reaches the grooves; 2nd, that it takes place after the light has passed them; and the results are very different according as one or other of the two rival theories is adopted. In the principal experiments, the plane of the plate was placed perpendicular to the incident light, and the light observed was that which had been diffracted by transmission through the plate. The angle of diffraction, by which is meant the angle measured in air, ranged in the different experiments from about 20° to 60°. The result obtained was, that when the grooved face was turned towards the eye, there was a very sensible crowding of the planes of polarization of the diffracted light towards the plane of diffraction. When the grooved face was turned towards the incident light, there was a considerable crowding in the same direction, much more than in the other case. Since the effect of refraction, considered apart from diffraction, would be to crowd the planes in the contrary direction, the result seemed decisive in favour of Fresnel's hypothesis, that the vibrations are perpendicular to the plane of polarization. On the other hypothesis, diffraction would have conspired with refraction to produce a large crowding in a direction contrary to that in which the observed crowding took place. The amount of crowding, in both positions of the plate, was nearly what would be given by theory, on adopting Fresnel's hypothesis, and supposing that the diffraction took place *before* the light reached the grooves, but appeared in both cases a little less. The difference, however, was comprised within the limits of uncertainty depending upon the errors of observation and the error in the assumed value of the refractive index of the glass plate.

December 10, 1849.

Impact on Elastic Beams. By Homersham Cox, Esq., B.A., Jesus College.

Among the experiments instituted by the Royal Commission appointed to inquire respecting the use of iron in railway structure, was a series relating to impact on beams. These experiments were undertaken by Professor Hodgkinson, and were conducted in the following manner. The two ends of the beam were fixed in a horizontal position, and the blow was given against one of its vertical sides, in a horizontal direction. The instrument for giving the blow

was a heavy iron ball, hanging down, when at rest, from a point of suspension vertically above the centre of the beam. The ball was raised through different arcs, and after descending by its own gravity, struck the beam. The deflection corresponding to different arcs of descent were carefully noted by a graduated scale.

The object of the present paper is to show that the results might have been predicted by known theoretical principles with considerable precision and confidence. The problem is divided into two parts:—1st, to estimate the amount of velocity lost by the ball at the first instant of collision; 2nd, to ascertain the effect of the elastic forces of the beam in destroying the *vis viva* which the whole system has immediately after collision. In the first part of the investigation, a general formula, derived from the combination of D'Alembert's principle and that of virtual velocities, is given for the motion of any material system subject to impact. The requisite geometrical condition required for the application of this general formula to the present case is obtained by the assumption, that immediately after impact the form of the beam is a gradual and tolerably uniform curve, such as, for example, the elastic curve of equilibrium. In this way it is determined that about one-half the inertia of the beam is effectively applied at the instant of collision to retard the ball.

The *vis viva* of the whole system thus computed is destroyed by the elastic forces of the beam developed by deflection. These, in the second part of the problem, are assumed to vary as the amount of central deflection. By the principle of *vis viva* a formula is easily obtained, connecting the amount of total deflection with the *vis viva* of the system immediately after collision.

Tables are given in which the theoretical and experimental results are compared. The correspondence is of the closest and most satisfactory nature. Indeed the theoretical result generally differs less from the mean of several experiments than those experiments differ among themselves. Both in the theoretical and experimental inquiries, every possible variation of the elements of the investigation —the relative masses of the beam and ball—the velocity of the latter —the rigidity and dimensions of the former—have been included.

February 11, 1850.

A paper was read by the Master of Trinity, "Criticism of Aristotle's account of Induction."

The passage criticised was *Analyt. Prior*. 11. 25, and is by Aristotle illustrated by this example. Elephant, horse, mule, &c., are long-lived; but elephant, horse, mule, &c. have no gall-bladder. If we suppose that the latter proposition may be converted and put in this form, "all animals which have no gall-bladder are as elephant, horse, mule, &c.," we may draw the conclusion that all animals which have no gall-bladder are long-lived. This convertibility and generalization of the second proposition are the necessary conditions for translating

induction into syllogism. And Aristotle really contemplated such a generalizing induction. He did not contemplate what has been called *inductio per enumerationem simplicem*, which is really no induction at all. This was shown to be so by reference to the case, often used as an example of induction, of the inference of Kepler's laws from the observation of the separate planets. It may be objected that the reasoning in such cases is inconclusive; and to this it is replied, that induction, as *reasoning, is* inconclusive. It is a source of truth different from reasoning; of first truths, the bases of reasonings, as Aristotle has remarked.

February 25, 1850.

On the Symbols of Logic, the theory of the Syllogism, and in particular of the Copula, and the application of the Theory of Probabilities to some questions of evidence. By Professor De Morgan.

This paper, which is in continuation of the one published in vol. viii. part 3 (read Nov. 9, 1846), and of subsequent additions contained in the author's work on *Formal Logic*, is divided into six sections.

Section I. *On the approximation of logical and algebraical modes of thought.*—The subjects of this section are,—1st, some development of the idea that the oppositions of logic have affinities which may one day lead to a connected theory, making use of a common instrument, just as the oppositions of quantity which are considered in algebra are connected by the general theory of the signs + and −; and 2nd, some remarks on the resemblance of the instrumental part of inference to algebraic elimination.

Ten such instances as affirmative and negative, conclusive and inconclusive, &c., are compared with the logical distinction of *universal* and *particular*; and it is pointed out, in all the cases in which it is not already acknowledged, that it would be *possible* to use any one of the ten in place of the last.

Section II. *On the formation of symbolic notation for propositions and syllogisms.*—Exclusive of remarks on the Aristotelian notation and on notation in general, and a statement for comparison of Sir William Hamilton's notation, this section contains the following matters.

1. A pictorial or diagrammatic representation of syllogistic inferences, being after the method pursued by Lambert, with such additions as will enable the system to represent all the cases in which contraries are used.

2. An abbreviated and arbitrary method of representing propositions and syllogisms.

Following Sir William Hamilton in making the quantity of both subject and predicate matter of symbolic expression, Mr. De Morgan gives his system of notation two new features. First, he dispenses with the representatives of the terms (except when it may be con-

venient to introduce them for the time), and represents the proposition by the *symbols of quantity only*, and the presence or absence of a sign of negation. Secondly, instead of making the symbols of universal and particular absolute, he gives one symbol,), to a universal subject and a particular predicate, and another, (, to a particular subject and a universal predicate: a dot [.] signifying negation. Thus X)·(Y, or simply)·(, represents 'No X is Y': X(·(Y, or (·(, represents 'Some Xs are not any Ys:' X()Y represents 'Some Xs are Ys.' Of the second circumstance above mentioned, Mr. De Morgan believes that it makes the rules easier, and knows that it makes the notation more suggestive.

Retaining in mind the order XY, YZ, XZ, which is the only *figure* used in the classification (being the first with inverted order of premises), the syllogism is to be denoted by the junction of the propositional symbols. Thus))))=)) denotes 'Every X is Y, every Y is Z, therefore every X is Z.' When this is to be read in any figure, the subject-quantities are to have their symbols thickened, the second premise being read first: thus in the four figures, in order, will be seen such symbols as ||||, ||||, ||||, ||||.

Section III. *On the symbolic forms of the extension of the Aristotelian system in which contraries are introduced.*—This system is the one which was completed and published to the Society *before* any correspondence with Sir William Hamilton. Mr. De Morgan remarks that it contains (incidentally, not designedly) every distribution of quantifications; and gives his reasons for not dwelling on this fact while the controversy was unfinished, with his statement that it had not struck him when the controversy began. Mr. De Morgan frequently distinguishes *this* system from Sir W. Hamilton's by calling the former that of *introduction of contraries*, the latter that of *invention of predicates*. For distinctness, it may be stated that Mr. De Morgan's other, or *numerically definite* system (the one concerned in the discussion), does not appear in the present paper, except as matter of allusion.

The forms of predication in this system are as follows, with reference to the order XY, *x* and *y* being not X and not Y.

Universals.

Affirmative	A_1	X))Y		Every X is Y
	A^1	*x*))*y*	or X((Y	Every Y is X
Negative	E_1	X))*y*	or X)·(Y	No X is Y
	E^1	*x*))Y	or X(·)Y	Everything is X or Y or both.

Particulars.

Affirmative	I_1	X()Y		Some Xs are Ys
	I^1	*x*()*y*	or X)(Y	Some things are neither Xs nor Ys
Negative	O_1	X()*y*	or X(·(Y	Some Xs are not Ys
	O^1	*x*()Y	or X)·)Y	Some Ys are not Xs.

Various rules of connexion are given, being all translations of those in the work on *Formal Logic*, except a classification of the particulars by *probability*, answering to that of universals. Thus of X))Y and X(·)Y, each makes the other *impossible*: of their con-

traries X(·(Y and X)(Y, each, so far as it affects the other, reduces its probability.

It appears that a quantified term has a quantified contrary: that of 'Every X' is 'some *xs*,' &c.

The symbolic *canon of validity* is;—if both middle parentheses turn the same way, there need be one universal proposition; if different ways, two. Thus)))(and (·))·(both have inferences; and so has)·()); but)·()(has none. The symbolic *canon of inference* is;—erase all signs of the middle term, and what is left (two negations, if there, counting as an affirmation) shows the inference. Thus from X(·)Y)·(Z we infer X(··(Z or X((Z: more simply, from (·))·(we infer ((.

Section IV. *On the symbolic forms of the system in which all the combinations of quantity are introduced by arbitrary invention of forms of predication* (Sir W. Hamilton's).

The modes of predication *peculiar* to this system have the same symbols,)(and (·), as the peculiar propositions of the system of contraries; but with very different significations, as follows:—

Contraries.	*Invention of predicates.*
(·) Universal negative with particular terms, and affirmative form in common language. *All things are either Xs or Ys.*	(·) Particular negative with particular terms, not used in common language. *Some Xs are not some Ys.*
)(Particular affirmative with universal terms, and negative form in common language. *Some things are neither Xs nor Ys.*	)(Universal affirmative with universal terms, being declaration of identity in common language. *All Xs are all Ys.*

Mr. De Morgan argues that Sir William Hamilton's system cannot be called an *extension* of that of Aristotle, in the sense in which that word is used.

The forms of predication are as follows:—

A_1+A^1	)(	All Xs are all Ys	E_1	)·(	No Xs are Ys
I_1	()	Some Xs are some Ys	—	(·)	Some Xs are not some Ys
A_1	))	All Xs are some Ys	O^1	)·)	No Xs are some Ys.
A^1	((	Some Xs are all Ys	O_1	(·(	Some Xs are no Ys.

Previously to entering upon the forms of syllogism, Mr. De Morgan repeats and reinforces the objections brought forward in his *Formal Logic*; namely, that)(is a compound of)) and ((, and has no simple contradiction in the system; and that (·) not only has no simple contradiction, but cannot be contradicted except when the terms are singular and identical. He then proceeds to propose one mode of remedying these defects. Calling the ordinary proposition *cumular*, he proposes to make it *exemplar*, as asserting or denying of one instance only. In the universal proposition, the example is *wholly indefinite, any one*; in the particular proposition it is *not wholly indefinite, some one*. The defects of contradiction are thus entirely removed, as in the following list, in which each universal proposition is followed by its contradiction.

)(Any one X is any* one Y	((Some one X is any one Y
(·) Some one X is not some one Y	)·) Any one X is not some one Y
)) Any one X is some one Y	)·(Any one X is not any one Y
(·(Some one X is not any one Y	() Some one X is some one Y

In both systems there are thirty-six valid syllogisms, and in both the canon of validity is,—one universal (or wholly indefinite) middle term, and one affirmative proposition. But the symbolic canons of inference differ as follows (with reference to the order XY, YZ, XZ).

Exemplar system.—Erase the middle parentheses, and the symbol of the conclusion is left: thus ())·) gives (·).

Cumular system.—Erase the middle parentheses, and then, if both the erased parentheses turn the same way, turn any universal parenthesis which turns the other way, unless it be protected by a mark of negation. Thus)·(() gives)·), ())(gives (), and ())·(gives (·(.

Section V. *On the theory of the copula, and its connexion with the doctrine of figure.*—In his work on *Formal Logic*, Mr. De Morgan had analysed the copula, and abstracted what he calls the *copular conditions* of the relation connecting subject and predicate. These are, *transitiveness*, seen in such copulæ as *support*, *govern*, *is greater than*, &c., *ex. gr.* if A govern B, and B govern C, A governs C: and *convertibility*, seen in such copulæ as *is acquainted with*, *agrees with*, &c.; *ex. gr.* if A agree with B, B agrees with A. Mr. De Morgan's position is, that any mode of relation which satisfies both these conditions has as much claim to be the copula as the usual one, *is*, which derives its fitness entirely from satisfying the above conditions. So far the work cited. In the present paper the *correlative* copula is introduced, as *is supported* in opposition to *supports*, &c., and every system of syllogism is thus extended. If a copula be taken which is only transitive, but not convertible, every syllogism remains valid, provided that the *correlative* of that copula be used instead of it, when needful. And in this consists, according to Mr. De Morgan, the root of the doctrine of *figure*. If + represent affirmative, and — negative, the four figures are connected with + +, + —, — +, and — — (in the system of contraries, where negative premises may have a valid conclusion, the fourth figure has equal claims with the rest, though the conditions of all the figures are singularly altered). These forms do not require the correlative copula: thus + — in the second figure (as *Camestres* and *Baroko* among the Aristotelian forms) are as valid when the copula is '*supports*' or '*is greater than*,' as when '*is*' is employed. But in every other case the rule for the proper introduction of the correlative copula is as follows:—The preceding being called the *primitive forms* of the four figures, when *one* premise of a primitive form is altered, the necessity of a correlative copula is thrown upon *the other*; when *both*, upon the conclusion. Thus, the primitive form of the second figure being + —, and *Cesare* showing — +, it is only valid with the copula '*governs*,' by making '*is not governed by*' the copula of the conclusion, as follows:—

No Z governs any Y
Every X governs a Y
Therefore no X *is governed by* any Z.

* So that there can be but one X and one Y, and that X is Y.

By an additional letter (g) introduced into the usual words of syllogism, the places of the correlative copula may be remembered, as in *Barbara, Celagrent*, &c.: a g being made to accompany any member of the syllogism in which the correlative copula must be employed.

This theory is applied equally to the Aristotelian system, to Sir William Hamilton's (though not of universal application in the *cumular* form), and to Mr. De Morgan's system of contraries. The extensions required by the use of a merely transitive copula, in the last-mentioned system, are discussed; and mention is made of the *tricopular* system, in which the leading copula and its correlative have an intermediate or middle relation, equally connected with both; as in $>$ $=$ and $<$ of the mathematicians.

The next step is the assertion that it is not necessary that any two of the three copulæ of a syllogism should be the same; all that is requisite is that, in affirmative syllogisms, the copular relation in the conclusion should be compounded of those in the premises. The instrumental part of inference is described by Mr. De Morgan as *the elimination of a term by composition (including resolution) of relations*, which leads to the conclusion that *whenever a negative premise occurs, there is a resolution of a compound relation*. This resolution is shown in a case (among others) of the ordinary copula, in which, however, it would hardly strike the mind more forcibly than would the properties of powers in algebra if every letter represented unity. Mr. De Morgan shows (in an addition) that in some isolated cases of inference which are not reducible to ordinary syllogism, logicians have had recourse to what amounts to composition of relations.

Mr. De Morgan next points out that the copular relation, in affirmative propositions, need not be restricted as applying to one instance only of the predicate; and shows that the removal of this usual restriction entirely removes *all* his objections to Sir William Hamilton's form of his own system.

Section VI. *On the application of the theory of probabilities to some questions of evidence.*—This inquiry was suggested by the apparent (but only apparent) error of the logicians, who seem to lean towards the maxim that, when the subject and predicate are unknown, the universal and particular propositions 'Every X is Y,' 'Some Xs are not Ys,' are *à priori* of equal probability. The difficulty is one which occurs in the following case:—If a good witness, drawing a card from a pack, were to announce the seven of spades, his credit would not be lowered, though he would have asserted an event against which it was 51 to 1 *à priori*. A common person gives the true answer, 'Why not the seven of spades as well as any other?' Many readers of works on probability would be inclined to say 'That is not the question; why the seven of spades rather than some one or another of the fifty-one others?' The retort is fallacious: it rubs out the distinctive marks from the other fifty-one cards, and writes on each of them 'not the seven of spades' as its only exponent. Laplace has chosen two problems, in one of which the distinctive marks exist, and not in the other; and, neglecting the consideration of the first one, has founded his remarks upon the deterioration of evidence

by the assertion of an improbable event, entirely upon the second. The object of this section is, by a closer examination of the mathematical problem of evidence, to ascertain the accordance or non-accordance of the results of usual data with usual notions. The result of the examination is, that common notions, as in other cases, are found closely accordant with theory. For instance, if there be n possible things which can happen, so that the *mean* probability of an event is $\frac{1}{n}$, a witness of whom we know no *particular bias* towards one mode of error rather than another, asserting an event of which the *à priori* probability is a, has his previous credit raised, unaltered, or lowered, according as $a-\frac{1}{n}$ is positive, nothing, or negative. So that though the *à priori* probabilities were distributed among a million of possible and distinguishable cases, yet a witness asserting one of them against which it is only 999,999 to 1, would have as good a right to be believed as though there had been but two equally probable cases, of which he had asserted one.

March 11, 1850.

On the Numerical Calculation of a class of Definite Integrals and Infinite Series. By Professor Stokes.

In a paper "On the Intensity of Light in the neighbourhood of a Caustic," printed in the sixth volume of the Cambridge Philosophical Transactions, Mr. Airy, the Astronomer Royal, has been led to consider the integral

$$W=\int_0^\infty \cos\frac{\pi}{2}(w^3-mw)dw,$$

and has tabulated it from $m=-4$ to $m=+4$ by the method of quadratures. In a supplement to the same paper, printed in the fifth part of the eighth volume, Mr. Airy has extended the table as far as $m=\pm 5\cdot 6$, by means of a series proceeding according to ascending powers of m. This series, though convergent for all values of m, however great, is extremely inconvenient for numerical calculation when m is large, and moreover gives no information as to the law of the progress of the function for large values of m. The author has obtained the following expression for the calculation of W for large, or even moderately large, positive values of m:

$$W=2\,(3m)^{-\frac{1}{4}}\left\{R\cos\left(\phi-\frac{\pi}{4}\right)+S\sin\left(\phi-\frac{\pi}{4}\right)\right\},$$

where

$$R=1-\frac{1.5.7.11}{1.2(72\phi)^2}+\frac{1.5.7.11.13.17.19.23}{1.2.3.4(72\phi)^4}-\dots,$$

$$S=\frac{1.5}{1.72\phi}-\frac{1.5.7.11.13.17}{1.2.3(72\phi)^3}+\dots,$$

$$\phi=\pi\left(\frac{m}{3}\right)^{\frac{3}{2}}.$$

When m is negative, and $+mw$ is written for $-mw$ in the integra

W, so that in the altered form of the integral m is positive, there results

$$W=2^{-\frac{1}{2}}(3m)^{-\frac{1}{4}}\varepsilon^{-\phi}\left\{1-\frac{1.5}{1.72\phi}+\frac{1.5.7.11}{1.2(72\phi)^2}-\dots\right\}.$$

By means of these expressions, W may be calculated with great facility when m is at all large. The author has given a table of the roots of the equation $W=0$, from the second to the fiftieth inclusively, calculated by a formula derived from the former of the above expressions. This formula was not sufficiently convergent to give the first root to more than three places of decimals; but this root may be obtained more accurately from Mr. Airy's table.

The method by which the author has treated the integral W appears to be of very general application, and he has further exemplified it by applying it to the infinite series

$$1-\frac{x^2}{2^2}+\frac{x^4}{2^2.4^2}-\frac{x^6}{2^2.4^2.6^2}+\dots=\frac{2}{\pi}\int_0^{\frac{\pi}{2}}\cos(x\cos\theta)d\theta,$$

which occurs in a great many physical investigations, as well as to the integral which occurs in investigating the diffraction produced by a screen with a small circular aperture, placed in front of the object-glass of a telescope through which a luminous point is viewed.

Curvature of Imperfectly Elastic Beams. By Homersham Cox, B.A. Jesus College.

The equation to the curve of an elastic deflected beam is usually deduced from the assumption,—1, that the longitudinal compression or extension of an elastic filament is proportional to the compressing or extending force; 2, that for equal extension and compression the compressing and extending forces are equal to each other.

These hypotheses are not quite correct in practice. All substances appear to be subject to a *defect of elasticity*, *i. e.* their elastic forces of restitution increase in a somewhat less degree than in proportion to the extension or compression. If the forces be taken as functions of the latter quantities expressed by a converging series of their ascending integral powers, the terms after the third may in general be neglected as of inconsiderable magnitude. If then e be the extension of a uniform rod of a unit of length and a unit of sectional area, the longitudinal force producing that extension is

$$\alpha e+\beta e^2+\beta' e^3,$$

where α, β, β' are empirical constants.

Similarly, if c be the compression of a similar rod, the force producing that compression is

$$\gamma c+\delta c^2+\delta' c^3,$$

where γ, δ, δ' are three other empirical constants.

These formulæ are to be applied to a uniform beam of rectangular section, resting on horizontal supports and slightly deflected at its centre. For this purpose, the compression and extension of every filament of the beam are expressed in terms of the radius of curvature and the distance from the neutral axis. Analytical expressions are thus obtained for the elastic forces developed in any transverse section of the beam; and the position of the neutral axis is obtained

from the integrals of these expressions by the principle, that the sum of all the horizontal forces above is equal to the sum of all the horizontal forces below the neutral axis.

Next, the sums of the *moments* of the elastic forces about the neutral axis are obtained; and the sums are equated to the moment about that axis of the pressure (P) of the fulcrum, the latter moment being the product of half the deflecting weight by the distances (x) of the fulcrum from the point of the neutral axis here considered. This equation involves the radius of curvature, and is solved with respect to the reciprocal of that quantity. It is to be observed, that this equation, and also the preceding one determining the neutral axis, are not of such a form as to admit of direct solution, and are therefore solved by an ordinary method of approximation.

The reciprocal of the radius of curvature of a point (x, y) of a curve is equal to (the second differential of y with respect to x)$\div$(a quantity which becomes equal to unity when, as here, the inclination to the axis of x of the tangent at any point of the curve is comparatively very small).

Making the substitution indicated, and integrating twice the equation last obtained, we obtained finally for the equation to the neutral line of a rectangular beam of vertical depth d, and horizontal breadth μ, and length $2a$,

$$y=\frac{\kappa x^3}{2.3}-\frac{b\kappa^2x^4}{3.4}+\frac{(2b^2-c)\kappa^3x^5}{4.5}-\left(\frac{\kappa a^2}{2}-\frac{b\kappa^2a^3}{3}+\frac{(2b^2-c)a^4\kappa^3}{4}\right)x,$$

where

$$b=\frac{3}{4}d(\beta+\delta\alpha^2\gamma^{-2})(1+\alpha^{\frac{1}{2}}\gamma^{-\frac{1}{2}})^{-2}$$

$$c=\frac{3}{5}\frac{d^2}{\alpha}(1+\alpha^{\frac{1}{2}}\gamma^{-\frac{1}{2}})^{-3}(\beta'+\delta'\alpha^{\frac{5}{2}}\gamma^{-\frac{5}{2}})$$

$$\kappa=\frac{P}{\mu}\frac{3}{d^3\alpha}(1+\alpha^{\frac{1}{2}}\gamma^{-\frac{1}{2}})^2.$$

If, according to the ordinary hypotheses of perfect elasticity, we put $\alpha=\gamma$ and neglect terms depending on β, β', δ, δ', this equation to the elastic curve coincides with that given by Poisson and others.

If we put $x=a$, the value of the deflection at the centre of the beam is

$$\frac{\kappa a^3}{3}-\frac{b\kappa^2a^4}{4}+\frac{(2b^2-c)\kappa^3a^5}{5}.$$

Whence it may be seen that the deflection is greater than it would be if the elasticity were perfect.

On the Knowledge of Body and Space. By H. Wedgwood, M.A.

No part of the great metaphysical problem chalked out by Locke has been more assiduously laboured, and none has attained a less satisfactory solution, than that which relates to the origin of the idea of space and its subordinate conceptions, figure, position, magnitude.

It was seen that the exercise of the muscular frame must somehow be instrumental in making us acquainted with the material and ex-

tended world, but all hopes of a logical explanation of the process by which that effect is produced seemed cut off at the outset by a preliminary objection. The knowledge of motion, it was said, obviously involves the knowledge of the body moved. The consciousness of the motion of the hand therefore implies the conception of the hand itself, an object of certain shape and size. The attempt to account for the notions of shape and size from the motion of the hand was thus apparently stranded in a hopeless paralogism; and so insurmountable was the difficulty taken to be, that philosophers were driven to imagine a second source of elementary ideas, in addition to the simple apprehension of the thing conceived in actual existence, maintaining that space is known to us as the *condition* under which we perceive external things, or, as others express it, that the notion of space arises in the mind on the first apprehension of body by a principle of necessary judgement, which impresses upon us the conviction that all body is contained in space.

In the paper laid before the Society, an attempt is made to show the utter barrenness of this hypothesis of a necessary origin (as it is called) of the idea of space; and the main object of the paper is to rest the idea on a more solid foundation, by showing the adequacy of muscular exertion, in conjunction with the sense of touch, to furnish us with complete knowledge of the material and extended world by the ordinary way of actual experience.

There are two kinds of action; one *instinctive*, immediately induced by the physical constitution of the agent independent of the understanding; the other *rational*, induced by the discernment of some object of desire in the end to be accomplished, and of course implying a previous conception of the action in question.

Familiar instances of instinctive action are then pointed out, from whence it would appear that the sensations of touch felt on contact of any part of the living frame with a foreign body operate as motives to instinctive exertion through the instrumentality of that part of the muscular frame on which the sensible impression is made, instinctively impelling the sentient being to muscular reaction against the material cause of the sensation, or leading him to shrink from it if the sensation is of a painful nature.

Attention is directed in particular to the action of an infant instinctively closing his hand upon a finger placed within his palm; and it is argued that the effect of such an action on his understanding will be the direct apprehension of *body*, a complex object consisting of *surface* (undeveloped as yet in form and magnitude) apprehensible by tactual sensation; and *substance*, revealed by resistance to muscular exertion, constituting a new kind of being essentially different from any of those discerned by means of the five senses.

The relation between body and space is illustrated by comparison with the case of light and darkness, the second of the two correlatives belonging in each case to Locke's class of *positive ideas from negative causes*. As he who has once apprehended light is subsequently enabled to look for that phænomenon in a direction from whence no rays actually penetrate the eye, so, it is argued, will he who has once made use of his hand in the apprehension of body be

enabled to stretch out the same member in feeling for body when none is actually within reach; and as in the former case the failure of the effort to discover light results in the sensible impression of black or darkness, so in the latter case the effort unsuccessfully aimed at the apprehension of body will take effect on the intelligence in the direct cognition or actual experience of space, viz. of that particular portion of space through which the hand is moved in the unsuccessful search after body.

Thus the notion of space, like that of body, or of any sensible phænomenon, is traced to the actual experience of the thing itself in concrete existence. The subsequent enlargement of the idea, so as to comprehend the space occupied by the solid substance of bodies and that which stretches away to infinity in all directions around us, is duly accounted for on the same principle; and that impossibility of conceiving the destruction of any portion of space, on which so much stress has been laid as establishing the necessity of a deeper-seated origin than simple experience, is shown to be the natural consequence of the negative foundation of the idea as explained by the analogy of light and darkness.

April 15, 1850.

On the Mathematical Exposition of some Doctrines of Political Economy. By the Master of Trinity.

The object of this paper was to solve algebraically certain problems which have been solved by Mr. J. S. Mill and others by means of numbers, taken as examples; the principles of these writers being taken for granted in the algebraical solution. Mr. Mill has rightly observed, that instead of saying that prices are determined by the *ratio* of demand and supply, we ought to say that they are determined by the *equation* of demand and supply. This equation may be thus stated. Let p be the price, and q the quantity bought and sold at that price. When p becomes p', let q become q'; and p' being equal to $p(1+n)$, let $p'q'=pq(1+mn)$: this is the equation of demand and supply. For different commodities, we have different values. There are such classes of commodities as these: (A.) *Conventional necessaries*, for which $m=1$: of these the same quantity is bought whatever be the price. (B.) *Articles of fixed expenditure*, for which $m=0$: on these the same sum is always expended, a smaller quantity being bought in proportion as they are dearer. (C.) *Common necessaries*, in which m is between 1 and 0: in these, when the price falls, the consumption is increased, but the money expended diminished. (D.) *Popular luxuries*, in which m is negative: in these, when the price falls, the consumption is so much increased that the money expended on them is increased also. For corn, the mean value of m seems to be about $\frac{1}{2}$: on this supposition a failure of one-fourth in the supply would double the price. The quantity m measures the susceptibility of the price to change when the supply changes, and also the intensity of the demand.

Another division of commodities is, according to the cost of production. These are (α) commodities of fixed and limited supply; (β) commodities of fixed cost; (γ) commodities of increasing cost for increasing supply, as for instance corn in a given limited district. The equation of price for the last case was given.

The like methods were applied to solve certain problems concerning international trade, treated by Mr. Mill. If the relative value of two commodities, C and D, in England and Germany be different, there will be a saving in exporting each from where it is cheaper to where it is dearer; and the question is, at what point prices will settle. We must introduce here the principle of *the uniformity of international prices*; namely, that when the trade is established, the relative prices of C and D will be the same in the two countries: the principle of the *equality of imports and exports* in each country; and the *equation of demand and supply* already stated. By combining these principles, the problem of the resulting price is solved. But it is found that there is no solution possible (that is, no solution in which both countries gain by the trade), except the *mutual demand* for the interchange of commodities be nearly equal. This limitation of the solution is given by the algebraical method, and seems to have been overlooked by previous writers.

The same methods were extended to a greater number of exported and imported commodities; and finally, it was remarked that these calculations are all founded on principles of equilibrium, whereas a state of equilibrium is never attained; and thus the theory may be very imperfectly applicable, like the equilibrium theory of the tides.

Second Memoir on the Intrinsic Equation of Curves. By the Master of Trinity.

The intrinsic equation of curves, according to which any curve is expressed by means of an equation between its length (s) and its angle of deflection (ϕ), may be conveniently used for many purposes. When a curve is so represented, the portion of the length which comes after a cusp must necessarily be taken as negative. This had appeared anomalous to some mathematicians, on the ground that a cusp is in all cases the limit of a loop. To clear up this point, the author adduces two cases. (1.) The curve of which the equation is $s=a\phi+b\sin\phi$, which is a looped curve when b is less than a, and a cusped curve otherwise. But in this curve it appears that a loop arises from the vanishing of *two* cusps, and of the intervening negative portion of the arc. (2.) The case of the ordinary trochoid, which is a looped curve when the describing point is exterior to the rolling circle, and becomes a cusped curve (a cycloid) when the point is in the circle. But in this case the length of the trochoid is equal to the length of an elliptical arc, which, in the case of the cycloid, coincides with the major axis, and becomes negative beyond the vertex of the ellipse. Other equations were examined, which give *running pattern* curves with cusps, cusped curves with infinite diverging spirals at the extremities, and sinuous curves with infinite converging spirals at the extremities; and certain integrals which occurred in the former memoir on this subject were discussed.

May 13, 1850.

Results* connected with the theory of the singular solution of a Differential Equation of the first order between two variables. By Professor De Morgan.

By a singular solution of a differential equation is here meant any solution which can be obtained by differentiation only, whether it be a case of the primitive by integration or not.

By a curve is meant all that is included under one equation, whether resoluble into what are commonly called complete curves or not. Thus, the equation

$$(x-y)(x^2+y^2-1)=0$$

belongs to *a curve*, having a rectilinear branch and a circular one.

By such a symbol as v_x is meant the partial differential coefficient $\frac{dv}{dx}$, when obtained from an equation in which v is explicitly expressed in terms of x and (it may be) other variables.

Let $\phi(x, y, c)=0$ be the complete primitive of the differential equation $y'=\chi(x, y)$.

$\phi(x, y, c)$ belongs to two distinct classes of curves:—

1. Continuous curves derived from such values of c, real or imaginary, as will enable $\phi=0$ to exist for points infinitely near to one another.

2. Systems of points, derived from

$$A(x, y, \alpha, \beta)=0, \quad B(x, y, \alpha, \beta)=0,$$

where

$$\phi(x, y, \alpha+\beta\sqrt{-1})=A(x, y, \alpha, \beta)+B(x, y, \alpha, \beta).\sqrt{-1}.$$

When a curve is such that the points on one side of it are on curves of the first kind, and those on the other side are part of systems of the second kind, let that curve be called a *separator*; and the same when it separates points of both kinds from points which belong to one kind only.

No solution of the differential equation can be formed by combining all those systems of the second kind in which α and β are connected by a real relation.

The curve which has at every point of it, either

$$\frac{\phi_x}{\phi_c}=\infty, \quad \frac{\phi_y}{\phi_c}=\infty,$$

or

$$\frac{\phi_x}{\phi_c}=\infty, \quad \frac{\phi_y}{\phi_c} \text{ finite}, \quad x=\text{const.},$$

* This communication is the abstract of a part of a paper not yet completed, and was forwarded to the Society for the purpose of ascertaining whether any examples could be produced destructive of the perfect generality of the results.

or $$\frac{\varphi_y}{\varphi_c}=\infty, \quad \frac{\varphi_x}{\varphi_c} \text{ finite}, \quad y=\text{const.},$$
is a singular solution. And in the above are contained *all* the singular solutions.

Every branch of a singular solution is either—

A separator, only.

A curve, every point of which has a contact of the first order at least with some one real primitive, only. Or both. Or neither.

If the first or last, it is a case of the complete primitive. And such cases may be introduced at pleasure into the singular solution, by writing the primitive in the form
$$\varphi(x, y, fc)=0.$$
A branch of a singular solution has at the utmost n contacts with each primitive which it touches (n being determined by the nature of the equation), and all of the first order, generally. Or, p_1 of the first order, p_2 of the second, &c., $p_1+2p_2+3p_3+\ldots$ being n or $n-$ (an even number); or some of these cases for some primitives, others for others, including the possibility of some cases giving none at all, when n is even.

The branches of the singular solution which have contact with ordinary primitives (whether themselves ordinary primitives or not) to the exclusion of the branches which are only separators, may be determined from the differential equation by the following test.

Let $y'=\chi(x, y)$ be the differential equation; whence
$$y''=\chi_x+\chi_y\cdot\chi.$$
Find the curves which satisfy either of the following sets of conditions:—
$$\chi_x=\infty \qquad \chi_y=\infty \qquad y'' \text{ finite},$$
or
$$\chi_x=\infty \qquad x=\text{const.} \qquad y'' \text{ finite},$$
or
$$\chi_y=\infty \qquad y=\text{const.} \qquad y'' \text{ finite}.$$
Every such curve *does* satisfy the differential equation, and is a *singular solution* having contact with some one primitive at every point.

And other such singular solutions there are none except those designated by $x=\infty$, or $y=\infty$, or both.

But if
$$\chi_x=\infty \text{ and } \chi_y=\infty \qquad y''=\infty,$$
or
$$\chi_x=\infty \qquad x=\text{const.} \qquad y''=\infty,$$
or
$$\chi_y=\infty \qquad y=\text{const.} \qquad y''=\infty,$$
then the differential equation may or may not be satisfied; but the curve passes through the singular points of the primitives, with or without contact, according as the differential equation is or is not satisfied. An evolute is such a pseudo-singular solution to all the involutes, passing through their cusps.

PROCEEDINGS

OF THE

CAMBRIDGE PHILOSOPHICAL SOCIETY.

November 11, 1850.

On the Mathematical exposition of certain Doctrines of Political Economy. By the Master of Trinity. Third Memoir.

The object of this memoir was to point out some of the laws of international trade, taking into account the effect of the import or export of money, *i. e.* of metallic currency. It was stated that when the balance of imports and exports is deranged by the import of a new commodity, previously produced at home, the effect is, to diminish the annual import of gold and silver; hence, to lower the scale of prices in general; hence, to increase the exports, and thus, to produce a new condition of equilibrium: and the necessary suppositions being made, the amount of depression in prices arising from such a cause was calculated.

The Master of Trinity also made a communication relative to *a new kind of coloured fringes*. He stated that he had, many years ago, remarked that if we hold a candle before a *dusty* looking-glass at a distance of six or eight feet, so that the image of the candle is near to that of the eye, the image of the candle is seen in the middle of a patch of coloured bars, which are perpendicular to the plane passing through the candle and the eye, normal to the looking-glass. This remark was communicated to M. Quetelet, and published by him. Attention has recently been drawn to this observation, at the Congress of Swiss men of science, held at Aarau, in August of the present year. M. Mousson of Zurich pointed out, at that meeting, the differences between the stripes noticed by Dr. Whewell, and the rings on specula observed by Fraunhofer. Among these differences are,—1st, Fraunhofer's rings depend upon the first surface of the speculum, the stripes upon both; 2nd, the rings are not produced except the dust be particles of uniform size; the stripes are produced by dust of irregular and various particles; 3rd, the rings depend for their size on the size of the particles of dust; the stripes do not.

Some discussion took place as to the manner in which these stripes arise from the theory of interferences, and upon their relations to Newton's "colours of *thick* plates."

December 9, 1850.

On the effect of the internal friction of Fluids on the Motion of Pendulums. By Professor Stokes.

It has been acknowledged for some time that the results which follow from the common theory of fluid motion relative to the effect of a fluid on the time of vibration of a pendulum do not agree well with observation. The volume of the Philosophical Transactions for 1832 contains the results obtained experimentally by the late Mr. Baily relating to the effect of air in altering the time of vibration of a great variety of pendulums. The experimental results are exhibited by the value of n, the factor by which the correction for buoyancy must be multiplied in order to give the whole effect observed. With pendulums composed of spheres suspended by fine wires, Baily found $n=1\cdot864$ for spheres a little less than $1\frac{1}{2}$ inch in diameter, and $n=1\cdot748$ for spheres about 2 inches in diameter. The result which follows from the common theory is $n=1\cdot5$, as was first shown by Poisson. The value 1·864 was the mean of 16 pair of experiments, giving a mean error 0·023, and 1·748 was the mean of 12 pair, which gave a mean error 0·014, so that the difference between the two results, and between either of them and the common theory, is far too large to be attributed to errors of observation.

The chief object of this paper was, to apply to the calculation of the motion of a pendulum the general equations of motion which are arrived at when the internal friction of the fluid is taken into account, and to compare the resulting formulæ with the experiments of Baily and others. The general equations, simplified, *first*, by neglecting the square of the velocity, *secondly*, by neglecting the compressibility of the fluid, the effect of which in the present instance is in fact quite insignificant, *thirdly*, by omitting the external forces, the effect of which may be taken into account separately, are

$$\frac{1}{\rho}\frac{dp}{dx}=\mu'\left(\frac{d^2u}{dx^2}+\frac{d^2u}{dy^2}+\frac{d^2u}{dz^2}\right)-\frac{du}{dt}, \text{ \&c.} \quad \ldots\ldots \quad (1.)$$

The second and third of the general equations are not written down, because they may be supplied by symmetry. In these equations ρ is the density, p the mean of the normal pressures in the direction of any three rectangular planes passing through the point of which x, y, z are the coordinates; u is the velocity in the direction of x, t the time, and μ' a certain constant, depending upon the nature of the fluid, which the author proposes to call the *index of friction*.

The author has succeeded in obtaining the solution of equations (1.) in the two cases of a sphere and of an infinite cylinder. The latter may be applied to the case of a pendulum consisting of a long cylindrical rod, by treating each element of the rod as belonging to an infinite cylinder oscillating with the same linear velocity. The following is the solution in the case of a sphere, so far as relates to the resultant action of the fluid on the sphere.

Let ξ be the abscissa of the centre of the sphere, measured in the

direction of the motion, a the radius of the sphere, τ the time of vibration, M′ the mass of the fluid displaced, F the resultant force of the fluid on the sphere, so that $-F$ is the resistance; then

$$-F=kM'\frac{d^2\xi}{dt^2}+k'\frac{\pi}{\tau}M'\frac{d\xi}{dt}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

where

$$k=\frac{1}{2}+\frac{9}{2}\left(\frac{\mu'\tau}{2\pi a^2}\right)^{\frac{1}{2}}, \qquad k'=\frac{9}{2}\left(\frac{\mu'\tau}{2\pi a^2}\right)^{\frac{1}{2}}+9\,\frac{\mu'\tau}{2\pi a^2}. \quad . \quad . \quad . \quad (3.)$$

The effect of a fluid on the time of vibration depends on the term which involves k; the effect on the arc of vibration depends on the term which involves k'.

The expression for F has precisely the same form (2.) in the case of a cylinder, but k and k' are certain transcendental functions of $(\mu'\tau)^{\frac{1}{2}}a^{-1}$ (a here denoting the radius of the cylinder), which the author has tabulated.

The value of μ' having been determined for air, or any given fluid, by one experiment giving the effect of the fluid either on the time of vibration, or on the arc of vibration, of any one pendulum consisting either of a sphere suspended by a fine wire, or of a long cylindrical rod, or of a combination of a sphere and a rod, the formulæ which follow from (2.) ought to make known the effect of the fluid both on the time and on the arc of vibration of all pendulums of the above forms. The agreement of theory with the experiments of Baily relating to the effect of the air on the time of vibration of pendulums is remarkably close. Even the rate of diminution of the arc of vibration, the observation of which held quite a subordinate place in Baily's experiments, agreed with the rate calculated from theory as closely as could reasonably have been expected.

The value of the index of friction of water was deduced by the author from some experiments of Coulomb's on the decrement of the arc of oscillation of discs which performed extremely slow oscillations in their own plane by the force of torsion. When this value was substituted in the expression for the time of vibration of a sphere, the result was found to agree almost exactly with Bessel's experiments on the time of vibration of a sphere swung in water.

As a limiting case of the problem of a ball pendulum, the author has deduced the resistance of a fluid to a sphere moving uniformly under such circumstances that the square of the velocity may be neglected. The resistance thus determined proves to be proportional, not to the surface, but to the radius of the sphere; and therefore the quotient of the resistance divided by the mass increases very rapidly as the radius decreases. Accordingly, the terminal velocity of a minute globule of water descending through the air depends almost wholly on the internal friction of air. Since the index of friction is known from Baily's pendulum experiments, the terminal velocity can be calculated numerically for a globule of given diameter. The velocity thus calculated proves to be so small, in the case of globules

such as those of which we may conceive the clouds to be formed, that the suspension of the clouds does not seem to offer any difficulty. Had the pressure been strictly equal in all directions in air in the state of motion, the terminal velocity of such globules would have been far larger, and consequently the quantity of water which could have existed in the air in the state of cloud would have been immensely diminished. It appears therefore that these small and hitherto almost unrecognized forces, which depend on internal friction, are essential to the fertility of at least the tropical regions of the earth.

The author has also applied the theory of internal friction to the calculation of the subsidence of a series of oscillatory waves. On substituting for the index of friction in the resulting formula the numerical value deduced from the experiments of Coulomb, it appears that in the long swell of the ocean the effect of friction is insignificant, whereas in the case of the short ripples excited on a small pool by a puff of wind the subsidence due to friction is very rapid. Accordingly, short ripples of this kind quickly die away when the breeze that excited them ceases to blow.

February 24, 1851.

On some points of the Integral Calculus. By Professor De Morgan.

Some time ago, Mr. De Morgan communicated to the Society an abstract of some unfinished views on the connexion between the ordinary and singular solution of a differential equation. The present paper completes those views, and also contains sections on the solution of differential equations by elimination, on the proof of the number of constants which a solution may contain, and on the criterion of integrability of a function of x, y, and differential coefficients of y.

1. *On singular solutions.*—As to equations of the first order, the tests obtained in this paper may be described as follows:—

Mr. De Morgan means by a *singular* solution any one which is obtained by other process than integration, whether it be contained in the integrated primitive, or not. When the singular solution is *not* contained in the primitive, he calls it an *extraneous* solution.

Let $\phi(x, y, c)=0$ be the primitive equation, giving $c=\Phi(x, y)$. The differential equation then is

$$y'=-\frac{\Phi_x}{\Phi_y} \text{ say}=\chi(x, y) \qquad \left\{\Phi_x=\frac{d\Phi}{dx}, \text{ \&c.}\right\}$$

$$\text{and } \Phi_x=-\frac{\phi_x}{\phi_c} \qquad \Phi_y=-\frac{\phi_y}{\phi_c}$$

Every relation between x and y which satisfies either of the fol-

lowing collective conditions, *is a solution of the equation*; and, by definition, a singular solution.

1. Φ_x and Φ_y both infinite.
2. Φ_x *only* infinite, and x=const.
3. Φ_y *only* infinite, and y=const.

And all possible finite solutions of the differential equation are given either by the original primitive, or by these relations.

Let $X=\dfrac{\chi_x+\chi_y\chi}{\chi_y}$. Then all relations between x and y which satisfy either of the following collective conditions are solutions of the differential equation, and are singular solutions.

1. χ_x and χ_y both infinite, and $X=0$.
2. χ_x only infinite, x=const., and $X=0$.
3. χ_y only infinite, y=const., and $X=0$.

But when one of these sets fails only in that X does not vanish, the curve so obtained, instead of having contact with a primitive curve at every one of its points, passes through the points of infinite curvature of the primitives; and the differential equation which is satisfied is $y'=\chi-X$. Every evolute is related in this manner to its involutes, passing through all their cusps.

The above tests do not give the possible case in which $x=\infty$, or $y=\infty$, is a singular solution.

Mr. De Morgan proposes the following geometrical illustration of the connexion between the primaries and the singular. Let c be the third ordinate of a surface (usually denoted by z) having the equation $\phi(x, y, c)=0$. The projections upon the plane of xy of sections parallel to that plane are the primaries: the singular solution is the base, upon the plane of xy, of a cylinder perpendicular to that plane, and which always touches the surface. By means of this illustration, it may be made manifest that certain cases of singular solution which have always been discarded as unmeaning, are limiting cases of the kind which are admitted in analysis so soon as the way up to the limit is clearly seen.

Taking a general equation with two arbitrary constants, so that a relation between those constants selects and designates a family of curves, it is shown generally (without examination of exceptional cases) how to find the families which have with their singular curves contact of the *second* order. The equation of these singular curves is a differential equation of the first order: but *its* singular solution is the singular curve of a family of curves which have with it a contact of the *third* order.

2. *Solution of differential equations by elimination.*—This is an idea derived from the method which Mr. De Morgan communicated (vol. viii. part 5) relative to partial differential equations, and which he found, after his paper was finished, had been given by M. Chasles, as he supposed, from knowledge of the *results* of Monge. But it afterwards appeared that the authority for Monge having obtained

such results is only a candid supposition *of M. Chasles himself*, and that no memoir on the subject, written by Monge, has been traced. All that M. Chasles had to proceed on was the *title* of a memoir mentioning a certain mode of generating conjugate surfaces, from which he thought it very likely that the solution of partial differential equations which he himself thence found, had really been found by Monge. Under these circumstances, Mr. De Morgan is of opinion that the method must be attributed to M. Chasles as its first discoverer, at least until something further appears.

Mr. De Morgan proceeds to make use of the equation

$$\int P.dy^{(n)} = Py^{(n)} - P'y^{(n-1)} + \dots$$

to form various cases of equations which can be reduced to lower orders, and which can finally be solved by elimination. Of these, the most simple specimen, being the one suggested by thinking on the method above alluded to, is as follows:—

If $x = Y'$ and $y = XY' - Y$, Y being a function of X, whence y is a function of x, we have the following sets of correlative equations:—

$$\begin{array}{ll}
x = Y' & X = y' \\
y = XY' - Y & Y = xy' - y \\
y' = X & Y' = x \\
y'' = \dfrac{1}{Y''} & Y'' = \dfrac{1}{y''} \\
y''' = -\dfrac{Y'''}{Y''^3} & Y''' = -\dfrac{y'''}{y''^3}
\end{array}$$

and so on. If, then, $\phi(x, y, y', y'', y''', \dots) = 0$ be a given differential equation, and if it be found that

$$\phi\left(Y', XY' - Y,\ X,\ \frac{1}{Y''},\ -\frac{Y'''}{Y''^3}, \dots\right) = 0$$

can be solved; it is seen that the original equation can be solved by eliminating X between $x = Y'$ and $y = XY' - Y$.

The general method of which this is a particular case, is as follows. Let $f(x, y, X, Y) = 0$ have its differential equations of the first order formed on two suppositions: first, that X and Y are constant; secondly, that x and y are constant. Let these differential equations be

$$\begin{array}{ll}
X = \Phi(x, y, y') & x = \phi(X, Y, Y') \\
Y = \Psi(x, y, y') & y = \psi(X, Y, Y').
\end{array}$$

These equations may be used instead of the first two pairs of correlatives in the preceding example: and each differential coefficient of Y is expressible by means of the same and lower differential coefficients of y; and *vice versâ*. To get convertible forms, as in the instance above, $f(x, y, X, Y)$ must be chosen so that x and y are simultaneously interchangeable with X and Y.

Mr. De Morgan gives a similar extension of the method as applied to partial differential equations.

3. *On the constants of a primitive equation.*—It is usually left to

be collected from induction that the equation of the nth order has n constants, and no more, in its complete primitive. Mr. De Morgan proposes an *à priori* proof of this point, on which, as in all such cases, it would be presumptuous to decide until it has been thoroughly examined.

He further proposes an extension of the meaning of the term *solution*, in the case of all the primitives intermediate between the differential equation and the original primitive. Thus, supposing an equation of the third order, of which the admitted primitives of the second order are

$$U_1=\text{const.},\quad U_2=\text{const.},\quad U_3=\text{const.},$$

he maintains that the general primitive of the second order is

$$f(U_1,\ U_2,\ U_3)=0,$$

where f is any function whatsoever: and, starting from this last equation, he determines a general primitive of the first order in a similar way.

This view is supported by the reduction of a common differential equation of the nth order to a partial differential equation of the first order with n independent variables.

4. *On the criterion of integrability of* $\phi(x,\ y,\ y',\ y'',\dots)$.—If we denote the differential coefficients of y by $p,\ q,\ r,\ s$, &c., it is well known that the condition which is both necessary and sufficient, in order that $V=\phi(x,\ y,\ p,\ q,\dots)$ may be integrable without reference to relation between y and x, is

$$V_y-V_p'+V_q''-V_r'''+\dots=0,$$

the accent denoting complete differentiation with respect to x. This has usually been established, either by the calculus of variations, or by a process of elaborate expression of the actual result in terms of definite integration with respect to a subsidiary variable. Mr. De Morgan, after some remarks upon the manner in which certain proofs of the *necessity* of the criterion fail, gives a very simple elementary proof founded upon the following theorem. If U be any function of $x,\ y,\ p$, &c.,—as far say as s, for an instance,—then

$$(U')_x=U_x',\ (U')_y=U_y',\ (U')_p=U_p'+U_y,$$
$$(U')_q=U_q'+U_p,\ (U')_r=U_r'+U_q,\ (U')_s=U_s'+U_r,\ (U')_t=U_s.$$

Mr. De Morgan takes it to have been hitherto unnoticed that the formulæ $V_p-V_q'+V_r''-\dots$, $V_q-V_r'+\dots$, so much used in this subject, are, when V is integrable, nothing but the differential coefficients of $\int Vdx$, with respect to $y,\ p$, &c.

[But since the paper was communicated, Mr. De Morgan has found the above theorem, and its consequences, in a memoir by M. Sarrus, apparently belonging to the *Journal de l'École Polytechnique*, and printed in 1824. No notice is taken of this method by MM. Bertrand, Binet, or Moigno, who have written on the subject since M. Sarrus.]

May 5, 1851.

Of the Transformation of Hypotheses in the History of Science. By W. Whewell, D.D.

The author remarks that new theories supersede old ones, not only by the succession of generations of men, but also by transformations which the previous theories undergo. Thus the Cartesian hypothesis of vortices was modified so that it explained, or was supposed to explain, a central force: and then, the Cartesian philosophers tried to accommodate this explanation of a central force to the phænomena which the Newtonian principles explained; so that in the end, their theory professed to do all that the Newtonian one did. The machinery of vortices was, however, a bad contrivance to produce a central force; and when it was applied to a globe, its defect became glaring. Still however, the doctrine of vortices has in it nothing which is absurd anterior to observation. The "nebular hypothesis" is a hypothesis of vortices with regard to the origin of the system of the universe, and is now held by eminent philosophers. Nor is the doctrine of the universal gravitation of matter at all inconsistent with some mechanical explanation of such a property; for instance, Le Sage's. We cannot say therefore that if the planets are moved by gravitation, they are not moved by vortices. The Cartesians held that they were moved by both; by the one, because by the other.

Like remarks may be made with respect to the theories of magnetism and of light.

May 19, 1851.

On the Colours of Thick Plates. By G. G. Stokes, M.A., Fellow of Pembroke College, and Lucasian Professor of Mathematics in the University of Cambridge.

By the expression "colours of thick plates" is usually understood the system of coloured rings, discovered by Newton, which are formed on a screen when the sun's light is transmitted through a small hole in the screen, and received perpendicularly upon a concave mirror of quicksilvered glass, placed at such a distance from the screen that the image of the hole is at the same distance from the mirror as the hole itself. The brilliancy of the rings, as was afterwards discovered, is greatly increased by tarnishing the surface of the mirror; and it is also advantageous to use a lens to collect the sun's rays, and to place the screen so that the small hole may be situated at the focus of the lens. These rings were first explained on the undulatory theory by Dr. Young, who attributed them to the interference of two streams of light, of which the first is scattered at the tarnished surface of the mirror, and then regularly reflected and refracted, while the second is regularly refracted and reflected, and then scattered in coming out of the glass. The theory has been worked out in detail

by Sir John Herschel, who has investigated the case in which the two surfaces of the glass belong to a pair of concentric spheres, and the hole in the screen is situated in the common centre of curvature.

A set of coloured bands has since been observed by Dr. Whewell in a common plane mirror. These bands are seen when a candle is held near the eye, at the distance of several feet from the mirror, and is viewed by reflexion. It is necessary that the first surface of the glass should be a little tarnished. The theory of these bands had not been worked out, and it had even been doubted by some philosophers whether they were of the nature of the coloured rings of thick plates.

In this paper the author gave a general investigation, which includes as particular cases the theory of the rings formed on a screen in Newton's experiment, and that of the bands which Dr. Whewell had observed in a plane mirror, and which are not thrown on a screen, but viewed directly by the eye. He also exhibited to the meeting a variation of Newton's experiment, in which an extremely beautiful system of rings is very easily produced without sunlight. The face of a concave mirror of quicksilvered glass was prepared by pouring on it a mixture consisting of one part of milk to three or four of water, and then holding the mirror vertically in front of a fire to dry. When the flame of a taper, or of an oil-lamp with a small wick, is placed in front of a mirror thus prepared, in such a position as to coincide with its inverted image, a beautiful system of rings is seen encompassing the flame. These rings appear to have a definite position in space, like a bodily object. The rings thus formed, which are evidently of the nature of Newton's coloured rings of thick plates, may be made to pass in a perfectly continuous manner into the coloured bands observed by Dr. Whewell.

The author has compared theory and experiment in various particulars, and has found the agreement perfect. It will be sufficient to mention here one result of theory, which is of great generality and of considerable elegance. It applies to the system of rings seen by reflexion in a mirror, either plane or curved, when a luminous point is placed anywhere near the axis, and the eye occupies any other position likewise near the axis. The result is as follows:— Join the eye with the luminous point, and likewise with its image, whether it be real or virtual, and find the points in which the joining lines, produced if necessary, cut the mirror. Describe a circle having for diameter the line joining these two points. This circle will be the middle line of the bright colourless fringe of the order zero, and on each side of it the colours will be arranged in descending order.

June 2, 1851.

On a new Elliptic Analyser. By Professor Stokes.

After mentioning some of the inconveniences and inaccuracies attending the use of a Fresnel's rhomb in the analysis of elliptically-polarized light, and alluding to some other methods which had been

employed for the purpose, the author proceeded to describe a new instrument which he had invented, and which he exhibited to the meeting. In the construction of this instrument he had aimed at being independent of the instrument-maker in all important points except the graduation. The construction is as follows:—

A brass rim or annulus is mounted so as to stand with its plane vertical when placed on a table. Within this rim turns a brass graduated disc; and the angle through which it turns is read off by means of verniers engraved on the face of the rim, and reading to tenths of a degree. This disc is pierced at the centre, and carries on the side turned towards the incident light a retarding plate of selenite, of such a thickness as to give a difference of retardation in the oppositely polarized pencils amounting to *about* a quarter of an undulation. In front it carries a hollow cylinder, turned on the lathe along with the disc itself. Round this cylinder there turns a collar containing a Nicol's prism, and carrying a pair of bevel-edged verniers, by which the angle may be read off through which the prism has been turned. Thus the retarding plate moves in azimuth carrying the prism along with it, and the prism has likewise an independent motion in azimuth.

In observing, the light is extinguished by a combination of the two movements, in which case the elliptically-polarized light is converted by the retarding plate into plane polarized, which is then extinguished by the Nicol's prism. On account of chromatic variations, the light is not, strictly speaking, extinguished, unless homogeneous light be employed, but only reduced to a minimum. There are two principal positions of the retarding plate and Nicol's prism in which the light is extinguished, or at least would be extinguished if the incident light were homogeneous; and for each principal position there are four subordinate positions, since either the retarding plate or the Nicol's prism may be reversed by turning it through 180°. The mean of the four subordinate positions may be taken for greater accuracy.

Let R, R′ be the readings of the fixed, r, r' those of the moveable verniers in the two principal positions; I the index error of the fixed verniers, that is, the azimuth of the major axis of the ellipse described, measured from a plane fixed in the disc; i the index error of the moveable verniers, that is, the azimuth of the principal plane of the prism, measured from a fixed plane in the disc; ϖ the angle whose tangent is equal to the ratio of the axes of the ellipse described; ρ the difference of retardation of the oppositely polarized pencils transmitted through the plate, measured as an angle, at the rate of 360° to one undulation. Then the unknown quantities I, i, ϖ, and ρ are given in terms of the known quantities R, R′, r, and r' by the following formulæ, which happen to be extremely convenient for numerical calculation:—

$$\mathrm{I}=\frac{1}{2}(\mathrm{R}'+\mathrm{R}); \qquad i=\frac{1}{2}(r'+r);$$

$$\cos 2\varpi=\frac{\sin(r'-r)}{\sin(\mathrm{R}'-\mathrm{R})}; \qquad \cos\rho=\frac{\tan(r'-r)}{\tan(\mathrm{R}'-\mathrm{R})}.$$

The author stated that he had already observed with this instrument, and after a little practice had found that it worked in a very satisfactory manner. When the light of the clouds was reflected horizontally by a mirror, and modified so as to produce elliptically-polarized light in which the ratio of the axes was about 3 to 1, it was found that the mean error of single observations amounted to about a quarter of a degree in the determination of the azimuth of the major axis, about three or four thousandths in the determination of the ratio of the minor to the major axis, and little more than the thousandth part of an undulation in the determination of ρ.

Since the magnitude of ρ depends upon the length of wave, or, what comes to the same, the refrangibility of the light, it follows that a knowledge of the former leads to a knowledge of the latter. It may thus be said that the instrument determines the azimuth and excentricity of the ellipse described, and the refrangibility of the light. An error of the thousandth part of an undulation in the determination of ρ would correspond to an error in the place in the spectrum assigned to the light operated on amounting to less than the twentieth part of the interval between the fixed lines D and E. Now by the use of absorbing media it is possible, without too much reducing the intensity of the light employed, to alter greatly its mean refrangibility; and yet for each medium the refrangibility may be determined very accurately by means of the value of ρ. Accordingly, the instrument is specially adapted for investigations relating to the dispersion of metals, and for other similar researches.

PROCEEDINGS

OF THE

CAMBRIDGE PHILOSOPHICAL SOCIETY.

December 8, 1851.

On the Oscillations of Suspension Bridges. By J. H. Röhrs, Esq., M.A.

In this paper the oscillations of a chain suspended at two points were discussed, with a view to explain the causes of fracture in suspension-bridges, by vibration arising from the tramping of troops, gusts of wind, &c., as well as to suggest means for obviating the mischief under those circumstances. The following were some of the most remarkable results arrived at:—

1st. That if the tension at the ends of the chain where it is suspended be kept constant by allowing play at those points, the variation of tension due to vibration at any other point of the chain will be but small.

2ndly. That if the chain be tied at the points of suspension so that it can have no motion there, a slight extent of vibration will produce comparatively a great increase of tension.

3rdly. That periodic forces, such as may be taken, for instance, to represent the effect of tramping in time of troops moving across the bridge, are dangerous in the extreme, as if they happen to coincide in period with any of the possible types of vibration, the extent of vibration will increase continuously, till it ceases to be represented approximately by a linear or even an equation of the second order; in this case, the chain will be divided by nodal points where there is no vertical motion.

4thly. That the mere transit, without tramping, of ordinary loads at an ordinary pace would not cause sensible vibration in a bridge of wide span; but that terms not periodic might be introduced by the variable pressure of wind sweeping in rapid gusts along the platform.

February 16, 1852.

On the Composition and Resolution of Streams of Polarized Light from different Sources. By Professor Stokes.

In this paper the author investigates the nature of the light resulting from the union of several independent streams of polarized light.

The refrangibility of the several streams is supposed to be the same, and the polarization to be of the most general nature, that is, to be elliptic. The following proposition is established.

When any number of independent polarized streams, of given refrangibility, are mixed together, the nature of the mixture is completely determined by the values of four constants, A, B, C, D, defined in the following manner:—Let J be the intensity of one of the elliptically-polarized streams, α the azimuth of its plane of maximum polarization, $\tan\beta$ the ratio of the axes of the ellipse described by the æthereal particles; then

$$A=\Sigma(J)\,;\ B=\Sigma(J\sin 2\beta)\,;\ C=\Sigma(J\cos 2\beta\cos 2\alpha)\,;$$
$$D=\Sigma(J\cos 2\beta\sin 2\alpha).$$

Two groups of polarized streams, of the same refrangibility, which are such as to give the same values to each of the four constants A, B, C, D, are defined to be *equivalent*; and the author has shown, that if two equivalent groups be transmitted through any optical train, and be afterwards analysed, they will present exactly the same appearance; so that equivalent groups may be regarded as optically identical.

It readily follows from the above theorem, that any group of polarized streams is equivalent to a stream of common light combined with a stream of elliptically-polarized light from a different source. If J, J′ be the intensities of these streams, α' the azimuth of the plane of maximum polarization of the latter, $\tan\beta'$ the ratio of the axes of the characteristic ellipse,

$$J=A-\sqrt{(A^2+B^2+C^2)}\,;\ J'=\sqrt{(A^2+B^2+C^2)}\,;$$
$$\sin 2\beta'=\frac{B}{\sqrt{(A^2+B^2+C^2)}}\,;\ \tan 2\alpha'=\frac{D}{C}.$$

The author has applied these formulæ to a few examples, and has likewise shown, from the general principles established in the paper, that the changes which are continually taking place in the epoch and intensity of the vibrations of polarized light may be of any nature. In the case of common light, the author contends that there is no occasion to suppose the transition from a series of vibrations of one kind to a series of another kind to be abrupt, but that it may be of any nature.

Professor Miller made a communication on the Artificial Formation of Crystallized Minerals.

March 1, 1852.

Mr. Hopkins, F.R.S. &c., gave a Lecture on the Influence of Internal Heat, Stellar Radiation, and Configuration of Land and Sea in producing Changes of the Earth's Superficial Temperature.

March 15, 1852.

Professor Miller made a communication on different improvements in the Reflective Goniometer; and a description of a New Reflective Goniometer.

Professor Stokes concluded a paper on the Composition and Resolution of Streams of Polarized Light from different Sources (see the abstract under the date Feb. 16, 1852, Phil. Mag. vol. iii. p. 316).

He also made a communication on Haidinger's Brushes.

Also on the Optical Properties of a New Salt of Quinine. The salt alluded to is that which had recently been discovered by Dr. Herapath (Phil. Mag. vol. iii. p. 161). The substance of this communication formed the subject of a notice of the properties of the salt which the author read at the Meeting of the British Association at Belfast, which will be found in the Report of the Transactions of the Sections.

April 26, 1852.

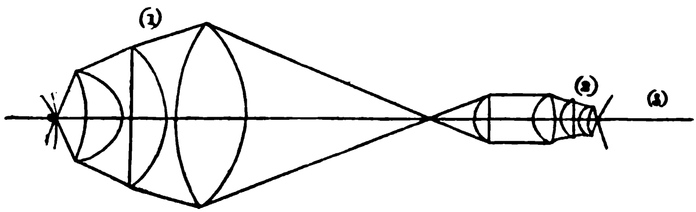
The Rev. Mr. Kingsley gave an account of the application of Photography to the Microscope.

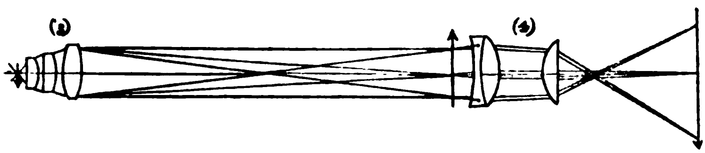
The earliest attempts in photography were directed both by Sir H. Davy and Mr. Fox Talbot to the fixing upon prepared paper the images of objects by the solar microscope, and the latter gentleman succeeded completely, as far as his instrument allowed, in obtaining pictures of minute structures. Shortly after the publication of Mr. Talbot's process, various attempts were made to apply the oxyhydrogen microscope to the same purpose, as that instrument had superseded the solar. The result however was, that it was abandoned on account of the great time that was found to be necessary for impressing an image; and after a great variety of trials by Prof. Owen, Dr. Carpenter, Dr. Leeson and others, the use of the instrument for this purpose was given up.

The discovery of the collodion process, so much more sensitive than that of Mr. Talbot, led the author to think, as soon as he became acquainted with it, that we were in possession of the means of impressing microscopic objects by means of artificial light without any great trouble. A friend of his had an oxyhydrogen microscope of the common form, and on making a trial with it, he found that by using a very sensitive kind of collodion, he could obtain images by about a minute's exposure. On examining the instrument, however, he saw that its form must be completely changed, in fact, that an entirely new kind of instrument was required to obtain the best effect. The two points to be regarded as the peculiar principles of this microscope are, 1st, that none of the radiant light be lost, or as little as possible; 2ndly, that the magnifying power be obtained by such means as would not place the screen for receiving the image beyond such a distance from the object, that the motions of the

instrument could be governed at the same time that the image was closely inspected. The first of these objects is secured by giving a very large angular aperture to the system of lenses used for collecting the light, and by using another set of lenses for condensing it again on the object, and so arranging their focal length in proportion to the focal length of the object-glass, as to cover the plate to be acted upon, and that space only: the second, by using a sort of eye-piece for enlarging the image formed by the object-glass.

The lenses divide themselves into four groups, as represented in the figures, in which the light is supposed to proceed from the left hand to the right. The first set for collecting the light is composed of three large lenses, a meniscus, plano-convex and double convex, being a combination of three lenses similar in effect to Herschel's doublet; the second set for condensing the light on the object is a





similar set of lenses, but of much shorter focal length, and turned the other way; between these two sets is a plano-convex or plano-concave lens placed at its focal length from the convergence of the rays from the first group, so as to make the rays pass to the condensers in a state of parallelism, and so do away with the necessity of changing the distance between the collectors and condensers for each adjustment of the latter: the third group forms the object-glass, which must be so corrected as to have the rays of the spectrum between the fixed lines G and H as much as possible brought to a point, as these rays are those that produce the maximum action on the silver salts used in photography; this will require the red rays to be left untouched, just in the same way as Fraunhofer left those of the blue end of the spectrum dispersed in correcting an object-glass for light. The fourth group is the common eye-piece left under-corrected. A rather better form for this is a Ramsden's eye-piece with the first lens partially achromatized, by making it a compound lens with the radius of curvature of the common surface nearly double that of the surface that would render it achromatic.

This form of eye-piece gives a better correction of the oblique pencils than the common negative.

The time of exposure to obtain an intense negative six inches diameter, on a collodion plate prepared as below, is about a minute; a positive is obtained in a fraction of a second.

The collodion is formed by dissolving gun-cotton in sulphuric æther, and adding to it a small portion of iodide of silver dissolved in iodide of potassium, and also a very small portion of bromide of iron, or of iodide or bromide of arsenic. The image is developed by protonitrate of iron, or by a solution of pyrogallic acid in acetic acid and water, and fixed by a solution of hyposulphite of soda.

By taking out the two first lenses of the collectors, the instrument is adapted for using sunlight.

Note.—At the time that this communication was made to the Society, Prof. Stokes had kindly made known to the author the results of his discoveries with regard to the rendering visible the chemical spectrum, but as he had not then made them public, the author of this communication could not state the use that Prof. Stokes's discovery enabled him to make of a screen composed of uranium glass, or of infusion of horse-chestnut bark, for finding the focal distance of the chemical image, or of arranging the lenses of the condenser so as to produce the maximum of chemical action.

Also, since the communication was made, it has been found that the instrument described gives light enough to impress an image on any of the ordinary papers or Daguerreotype plates in periods ranging between one and five minutes, with the oxyhydrogen and lime light; and with direct sunshine the impression is almost instantaneous; of course sunlight is much better than any artificial light when it can be procured, both as regards speed and the clearness of the picture produced.

May 10, 1852.

Professor Miller gave an account of a new method of adjusting the Knife-edges of a Balance.

Also of a method of determining the height of clouds by night.

May 24, 1852.

Professor Stokes gave a Lecture on the Internal and Epipolic Dispersion of Light.

November 8, 1852.

Mr. Adams, F.R.S. &c., gave an account of some Trigonometrical Operations to ascertain the difference of geographical position be-

tween the Observatory of St. John's College and the Cambridge Observatory.

The observations, especially those of eclipses and occultations, which were made during many years by the late Mr. Catton at the Observatory of St. John's College, and which have recently been reduced under the superintendence of the Astronomer Royal, render it a matter of some importance to determine the exact geographical position of that Observatory. The simplest and most accurate means of doing this appeared to be, to connect it trigonometrically with the Cambridge Observatory. For this purpose, a base was measured along the ridge of the roof of King's College Chapel, by means of two deal rods terminated by brass studs, the exact lengths of which were determined by comparison with a standard belonging to Professor Miller. The extremities of the base were then connected by a triangle, with a station on the roof of the Observatory at St. John's, from which, as well as from the two former points, a signal post on the roof of the Cambridge Observatory could be seen. The angles at the extremities of the base, combined with the corresponding ones at the station at St. John's, furnished two determinations of the distance of the Cambridge Observatory, which served to check one another. The meridian line of the transit instrument at St. John's passes through King's College Chapel, so that by observing the point at which it intersected the base, the azimuths of the sides of the triangles could be immediately found.

The result thus obtained is, that the transit instrument of the Cambridge Observatory is 2313 feet to the north, and 4770 feet to the west of that at St. John's College. Hence it follows that the difference of latitude is $22''\cdot 8$, and the difference of longitude $5''\cdot 10$; and the latitude of the Cambridge Observatory being $52^\circ\ 12'\ 51''\cdot 8$, and its longitude $23''\cdot 54$ east of Greenwich, we have finally for the geographical coordinates of the Observatory of St. John's College,

Latitude.. $52^\circ\ 12'\ 29''\cdot 0$
Longitude $0^\circ\ 0'\ 28''\cdot 64$ E. of Greenwich.

These operations, of course, furnish incidentally, a very exact determination of the orientation of King's College Chapel. The line of the ridge of the roof points $6^\circ\ 20'\cdot 3$ to the north of east.

November 22, 1852.

Professor Challis made a communication on the recent return of Biela's Double Comet.

December 6, 1852.

Professor Stokes gave an account of M. de Sénarmont's Researches relating to the Doubly-refracting Properties of Isomorphous Substances.

February 7, 1853.

An addition was read to a paper by Professor De Morgan on the Symbols of Logic, the Theory of Syllogism, &c.

A paper was read by Mr. Denison on some Recent Improvements in Clock Escapements.

The object of this paper was to explain the construction of a new *remontoire* or *gravity escapement* invented by the author, which has now been in action for some time on the pendulum of the great clock for the houses of parliament, and is in course of application to others, both turret clocks and astronomical.

But by way of introduction to this, which may be called the *three-legged gravity* escapement (from the form of the scape-wheel), Mr. Denison gave a description of another, which would similarly be called the *three-legged dead* escapement, and had been previously invented by him for the purpose of giving the impulse to the pendulum with far less friction than usual. He found that it required only $\frac{1}{5}$th of the force which a common dead escapement had required to make the pendulum swing the same arc. And therefore, as compared with a gravity escapement in which there is no sensible friction on the pendulum, there must be still more than $\frac{4}{5}$ths of the force in a common dead escapement wasted, in first producing friction on the pendulum, and then overcoming it by an increased impulse. The time of the pendulum would be much more disturbed than it is by the inevitable variations of this large amount of friction, as well as that of the clock train, but for a fortunate tendency of the different errors, which are caused by these variations of force and friction, to correct each other.

But the amount of this self-correction is uncertain, and sometimes one set of errors preponderates and sometimes the other; and so a dead escapement clock sometimes gains and sometimes loses simultaneously with either an increase or a decrease of the arc of vibration. And, consequently, none of the contrivances for isochronizing a pendulum for different arcs can secure isochronism of the clock; and no further material improvement in clocks can be expected, but from the solution of what has long been known as the great problem of clock-making, viz. the invention of a simple escapement which will give a constant impulse to the pendulum without any sensible friction.

Mr. Denison showed that his new gravity escapement satisfies all the requisite conditions, mechanical, mathematical, and œconomical. Its principal features are, that the scape-wheel has only three pins, not far from the centre, which lift the pallets or gravity-arms, and three long teeth which are locked by stops on the arms. The velocity of the scape-wheel, which usually produces *tripping*, if the force of the train is increased beyond what is just enough to lift the arms, is moderated by a fan-fly set on the axis of the scape-wheel. The arms are necessarily longer in this than in any other gravity escapement, and this also gives a greater depth of locking within a given angle, and therefore a still further security against tripping. And if an arm is by accident lifted a little too high, the tooth does not

escape, and the arm falls down again to its proper height until the pendulum carries it off, the pressure of the long teeth on the stops not being enough to hold it up. For these reasons also there is no difficulty in satisfying the mathematical condition investigated by Mr. Denison in a paper read before the Society in 1848, viz. that γ (the angle at which the pendulum leaves one arm and takes up the other) should $=\frac{a}{\sqrt{2}}$, or at any rate not be less than $\frac{a}{3}$ (a being the extreme arc of vibration). The escapement requires no oil in the parts affecting the pendulum; and it contains no delicate work, and is very easy to make; and as a highly finished train will be no longer necessary, astronomical clocks may be made on this plan much cheaper, as well as better, than heretofore.

In turret clocks an escapement of this kind supersedes the necessity for a remontoire in the train to equalize the force on the scape-wheel, and also of long and heavy pendulums, which are expensive when compensated, and are sometimes difficult to fix. It will also allow cast-iron wheels to be used throughout the clock (which Mr. Dent has now used for several years in connexion with Mr. Denison's spring remontoire for the train), as the friction of the train can no longer affect the pendulum.

February 21, 1853.

Professor Challis gave a Lecture on Halos, Parhelia, and Paraselenæ.

March 7, 1853.

Professor Stokes gave an account of some further researches relating to the Change of Refrangibility of Light.

April 11, 1853.

The Rev. Mr. Pritchard, F.R.S., gave an account of the Processes requisite to render Quicksilver tremorless for Astronomical Observation.

The great improvements recently introduced, and especially by the present Astronomer Royal, in the construction and methods of using astronomical instruments, require a far more extended use of reflexion from mercury than heretofore. Unfortunately, however, both the convenience and the accuracy of these methods have been greatly limited and impaired by the tremors to which mercury is liable. Many attempts have been made both in France and in Germany to remove or obviate these tremors, but hitherto by no means with perfect success. The Rev. C. Pritchard, of Clapham near London, has proposed a method which appears fully adequate to the requirements of astronomy. It consists in the adoption of a silver-plated or amalgamated copper vessel of a peculiar form, admitting the use of a very thin stratum of mercury without the necessity of an inconvenient amount of shallowness in the vessel itself. Mercury,

however, placed in an amalgamated vessel after a short time becomes covered with a singular film of amalgam, which impairs the reflecting power of the surface, and if at all agitated, soon entirely destroys it. And this is the case even when the vessel is made of amalgamated platina. The most important, and by far the most difficult part of Mr. Pritchard's experiments, consisted in the invention of a method by which these films can be easily and practically removed. The details, many of which are curious and interesting, would here occupy too much space, but they are fully explained in a memoir recently read to the Royal Astronomical Society of London; and it may be added, that the process has been adopted at the Royal Observatory at Greenwich, and is now in progress of trial at the Observatories of Paris and Cambridge.

April 25, 1853.

Professor Challis gave a lecture on the Adjustments of a Transit Instrument.

A paper was read by Professor De Morgan on the Principle of Mean Values, and an addition to a paper on the Symbols of Logic, &c. in vol. ix. part 1 of the Society's Transactions.

Though the heading of this paper describes one of its main results, yet it might with equal propriety have been styled a discussion of some points of algebra, with reference to the distinction of form and matter. This distinction, it is contended, is more extensively *applied* in algebra than in logic, though more *recognized* in logic than in algebra. Looking at the disputed points which exist in the higher parts of mathematics, and feeling satisfied that they will never be settled until the separation of form and matter is both visible and complete, the author makes a first attempt towards the examination of the question how far this yet remains to be done. A number of comparisons are made between algebraical and logical process, in the course of the inquiry, illustrative of the opinion entertained by Mr. De Morgan, that logic, as treated, requires the interposition of the algebraist, and cannot, except by aid of algebraical habits, be rendered a complete exposition of the forms of thought. In digressive notes, he combats the opinion that a generalization of the quantity is, as asserted, a new material introduction. He argues against the *too mathematical* tendency of some of the logicians who have endeavoured to extend the ancient system, especially the attempt of some to make the logical import of the proposition *nothing but* a comparison of more and less, and an equation or non-equation of quantities. He points out that the proposition has been formalised in nothing but its terms, subject and predicate; and gives an instance of the method in which a failure of general maxims is answered by the sole assertion that the mode of expression which brings about the failure is useless. He refers to what were called *sophisms*, contrasting the neglect of them by the logicians with the use which the algebraists have made of their corresponding difficulties, as in the case of negative and imaginary quantities, the fraction $\frac{0}{0}$, &c. He argues against the assertion of more than one eminent writer on logic, that the

identity of two terms, X and Y, expressed as in "all X is all Y," is *not* a complex proposition—is *not* the union of Every Y is X with Every X is Y. In an appendix to a former paper on the symbols of logic, he refers to a complaint of misrepresentation made by Sir W. Hamilton of Edinburgh, to whom certain technical phrases had been attributed. Mr. De Morgan makes the requisite correction, affirms that he had good reason for attributing such phraseology, and points out what that reason was: he then proceeds to answer two *new* charges of plagiarism against himself, from the same quarter; giving as his reason for addressing such answer to the Society, that Sir W. Hamilton makes the appearance of the asserted plagiarisms in the Transactions his principal ground of notice.

Finally, as to the logical part of the communication, Mr. De Morgan, reverting to his *complex* syllogism, in which each premise and the conclusion contain *two* ordinary propositions, generalizes the premises into the numerical form, and, giving terms and quantities algebraical designations, points out the mode of producing all possible inference. The immediate occasion of this introduction is as follows:—Sir W. Hamilton, in a recent publication, one tract of which is directed against Mr. De Morgan's last paper on syllogism, affirms that a proposition, as to its logical force, is *merely* an equation or non-equation of quantities, from which the declaration of coalescence or non-coalescence of terms into one notion is a *consequent*. Mr. De Morgan maintains the converse; namely, that the proposition is a declaration of coalescence or non-coalescence, of which the equation or non-equation of quantities is an essential. In treating the complex syllogism, under definitely numerical quantities, he has to search for the properties of the *equation of coalescence*, as distinguished from the *equation of quantity*; and, having made the former the means of arriving at inference, he invites those who can to try if the same result can be produced by means of the latter alone.

To pass to the algebraical part of the paper. It is first contended that the states *infinity* and *zero*, whether represented by distinctive symbols attached to 0 and ∞, or by negative and positive powers of dx, must be formally distinguished, as being each, not a value, but a *status*, containing an infinite number of *corrational* values, just as happens in finite quantity. In order to lay down the formal laws of connexion of these different states, it is necessary to examine the formal use of the symbol $=$. After pointing out instances in which the laws of algebra are by many declared invalid, as by those algebraists who admit and interpret $2x=x$, but cannot give permission to divide both sides by x, the following laws are suggested. The symbol $=$ is to be read with an index, as in $=_n$, which has reference to the order ∞_n or 0_{-n}, or as in $=_{-n}$, which has reference to ∞_{-n} or to 0_n. The equation $A=_nB$ is *normally* satisfied when A and B are of the order n, and $A-B$ of a lower order. It is *supernormally* satisfied if A and B be both of any (the same or different) higher order than the nth, and *subnormally* if both be of any lower order. Among the most conspicuous rules which follow, are that $AC=_{m+n}BD$ is normally satisfied, if $A=_mB$ and $C=_nD$ are so; and that when an

equation is multiplied or divided by a quantity of the order n, the index of equality must be increased or diminished by n. Various cases are given in which such results as now present anomalies are reduced under formal law, and others which would be absolutely rejected are shown to be capable of consistent interpretation.

The formal law of connexion of the different states, of which finitude (with the index 0) is only one, is that the order 0_m stands to finite quantity in all respects as finite quantity to ∞_m. Hence, so far as 1 and $1+0$ are *simultaneous* as well as equal, so far ∞ and $\infty+a$ are *simultaneous* as well as equal. And if $\phi(1)={}_0\phi(1+0)$ be a universal law, so must be $\phi(\infty)={}_0\,\phi(\infty+a)$. Further, $\infty-\infty$ must be, formally speaking, wholly indeterminate, even when it is a case of $x-x$.

In relation to such indeterminate forms as $\infty-\infty$, $\frac{0}{0}$, &c., Mr. De Morgan contends that their formal and *à priori* character is that of indeterminateness; and that the choice between determinate and indeterminate character, which so often occurs, is dictated by the *matter of the problem*, the determinate value being dictated by the laws of algebra. The index of equality, for instance, may be the means of decision: an example is given in which one equation belongs to two different problems, but with different indices of equality; in one $\frac{0}{0}$ is determinate, in the other wholly indeterminate.

In assigning ∞ or 0 as *values*, it is often necessary to assign relations of order. When a quantity passes from positive to negative, or the converse, through 0 or ∞, it passes through every order of 0 or ∞; and this even when the passage is from one phase of 0 or ∞ to another, of different signs. Thus, the orders being powers, x cannot pass from $-a\,.\,0^m$ to $+a\,.\,0^m$, without passing through even 0^∞.

Mr. De Morgan insists upon one of two things: either, the abandonment of the separate use of 0 and ∞, except only in the retention of the former symbol to represent $A-A$; or, the introduction of different orders, and the free use of the comparisons of those orders. For himself, he prefers and adopts the latter alternative.

The principle of limits is considered as a formal law of algebra, but not to the exclusion of every other result. If a constant, for instance, have the value A up to $x=a$ exclusive, it has A for one value when $x=a$. If the constant be *transitive*, that is, if it be always $=$B after $x=a$, then $x=a$ gives *both* A and B for the constant, and, as a *fact* hitherto observed, its value from calculation is $\frac{1}{2}(A+B)$. This observed fact Mr. De Morgan believes he connects with the principle of limits, making it a necessary consequence of the universal truth of that principle; and hence he holds that it may be stated as a theorem, under the name of the *principle of mean values*. Various uses of this principle are given. Further, in assuming the free use of the orders of 0 and ∞, it is shown that it is correct to say that the constant passes from A to B while h, x being $a+h$, passes through the phases of 0. So that, for instance, at an epoch of transitiveness the value of $\phi(a+0)$ is dependent upon the form of 0. The brevity of an abstract prevents the statement of those cautions under which such use of language is introduced. One

result, however, may be brought forward. When the function fx is transitive (or, as commonly said, discontinuous) at $x=a$, the equation $\phi(fa)=(\phi f)a$ no longer necessarily exists. But this, as is pointed out, is what may happen at any value of x which makes a differential coefficient infinite.

On the question of $\sin\infty$ and $\cos\infty$, Mr. De Morgan deduces their *observed* values, $\sin\infty=0$ and $\cos\infty=0$, both from the principle of mean values, and from the formal truth of the equation $\phi(\infty+a)={}_0\phi\infty$. From the same principles follows the equation $(-1)^{\infty}=0$. In this case, however, and in all which come under the principle of mean values, the absolute necessity of the results is not affirmed. They are the *alternatives of indeterminateness*. But in thus representing them, Mr. De Morgan does not concede more than he conceives must be conceded with respect to $\infty-\infty$, $\frac{0}{0}$, and the like.

On the question of series, Mr. De Morgan contends that all the uncertainty and danger of divergent series belongs equally to convergent series, in every case in which the envelopment is unknown. On this part of the subject he adds to the arguments of a former paper, and insists upon the superior safety of the *alternating* series, in which the terms are alternately positive and negative.

Without going further into details, the purport of this paper may be stated as follows. Algebra, using the term in the widest sense, ought to be, and is approaching towards, a science of investigation, and a symbolic art of expression, of which the laws are strictly and without exception incapable of failure, suspension, or modification. The formal laws under which such a result is to be obtained, though laid down in the first instance by extensive induction, of which many steps are accompanied by difference of opinion, will at last be received and admitted as parts of the definition of the science, *à priori*. The existing defect of the science is an imperfect formalization, arising from the want of views of sufficient extent, and leading to *material* distinctions, that is, to exceptions dictated, *à posteriori*, by the results of particular cases. Such exceptions have in many instances been brought within rule by further consideration; and it is conceived that the same thing will happen at last in all cases. The paper is an attempt to examine the principal outstanding difficulties (those connected with the definition of integration excepted) with reference to the question how far they may arise from imperfect conception of formal laws. That there is to be a formal science, is positively assumed, and made the basis of the attempt: how far any suggestion contained in the paper is a valid step towards it, is treated with doubt and left to opinion.

May 9, 1853.

Mr. Hopkins, F.R.S. &c., the President of the Society, gave an account of some experiments for the determination of the temperature of fusion of different substances under great pressure; and on the application of the results to ascertain the state of the interior of the earth.

PROCEEDINGS

OF THE

CAMBRIDGE PHILOSOPHICAL SOCIETY.

November 14, 1853.

A paper was read by Mr. Dobson on the Theory of Cyclones. See Philosophical Magazine, vol. vi. p. 438.

Also, on the Storm-tracks of the South Pacific Ocean. See Philosophical Magazine, vol. vii. p. 268.

A communication was made by Mr. C. C. Babington on the use that has been made of the mode of growth to distinguish nearly allied Species.

November 28, 1853.

A paper was read by Mr. Wedgwood on the Geometry of the first three books of Euclid, synthetically demonstrated from premises consisting exclusively of definitions.

In a treatise* published by the author a few years ago, definitions founded on relations of direction were indicated as exhibiting the ultimate analysis of the conceptions of straightness and parallelism in lines, and of planeness in surface; and in proof of the adequacy of these definitions as the basis of a complete system of geometry without the aid of axioms or any other assumption whatever, they were employed in demonstrating the principal propositions necessary to place the student on the ground occupied by the definitions and axioms of the ordinary system. If the basis thus built in underneath the old foundations of the science had been complete in every nook and corner, nothing more would have been required in order to rest the entire demonstration on the single principle of definitions. So long, however, as any step in the process, however subordinate, was left to be supplied by others, there always would be room for suspicion that the assumption in reasoning which was speciously plastered over in one place might be secretly undermining the system in another. The reform, moreover, of the premises in geometry is a problem on which such an infinity of thought has been spent, and

* The Principles of Geometrical Demonstration deduced from the original conception of Space and Form. Taylor and Walton. 1844.

to which so many answers, more or less plausible, have been offered, that nothing short of a complete exposition of a consistent scheme of demonstration can be expected to carry conviction in the validity of a fresh solution. The object of the present paper is accordingly to complete the task undertaken in the foregoing publication by a formal statement of the other definitions required in connexion with those of straight and parallel lines and plane surface, and by a rigid demonstration from these premises of the steps intervening between those and the premises of the ordinary system; and in additional proof of the fundamental character of the proposed analysis, the demonstration is carried on through the geometry of the three first books of Euclid by direct reasoning, without resort to the comparatively unsatisfactory method of *ex absurdo* proof, which, although equally conclusive as to the necessity of the result, yet always leaves a hankering in the mind for an answer why the case must be as the demonstration shows that it cannot avoid being.

In the execution of the foregoing plan, the whole of the problems of Euclid are omitted as irrelevant to the demonstration of the other propositions. The grounds on which they were adopted in the system of Euclid appear to be these. It frequently happens that it is necessary in the course of demonstration to make some new construction not included in the figure which forms the original subject of the proposition, and it was evidently thought that the geometer would not in strictness be entitled to take such a step until he had demonstrated the means of executing it with exactitude. The student was accordingly in the postulates put in possession of a ruler and a pair of compasses; and wherever any additional construction was required in the proof of a proposition, a problem was premised, showing the means by which the construction might be made by the aid of those implements.

But it should be recollected that the figure by which the demonstration is commonly accompanied is not the actual subject of the reasoning, but a mere illustration to aid the imagination and the memory, the exactitude of which is matter of comparative indifference. Moreover, the principle on which the problems are introduced is not consistently carried out to its legitimate conclusion even in Euclid. There is no difference in the reasoning between the figure which forms the original subject of the proposition, and the additional construction which is made in the course of demonstration; and therefore if it were necessary for the validity of the conclusion to demonstrate the means of executing the latter figure, it would be equally necessary in the case of the former. The student would not be entitled to move a step in the demonstration of the equality of two triangles having two sides and the included angle equal, until he had been taught how to construct two such triangles, and consequently how to describe an angle equal to a given angle. The demonstration in Euclid begins with perfect legitimacy. "Let ABC, DEF be two triangles in such and such conditions," without the necessity of indicating the means by which those conditions may be mechanically executed, or indeed of their possibility of actual exist-

ence; and it may with equal legitimacy proceed to exemplify in like manner any further construction which may be found necessary in the course of demonstration.

The question of motion has commonly been considered so essentially distinct from that of position, that all reference to the former subject has rigorously been excluded from the field of geometrical inquiry. But the position of every point must ultimately be determined by motion from points antecedently known, and to the incidents of motion we should accordingly look for the original source of the relations of position. Now motion (in as far as it influences position) admits of variation in two ways; viz. in the direction of the motion at each indivisible instant of time, and in the length of the track accomplished in a finite period; whence it has been said by Sir John Herschel that space (which is primarily known as the receptacle of motion) is reducible in ultimate analysis to distance and direction.

The relations of extent are simply those of equal, greater, and less, with respect to which it will be necessary only to define the test by which they are respectively to be demonstrated in concrete figure. The relations of direction are of a much more complicated nature. The different phases of this elementary attribute of motion are distinguished, not, like those of colour, by a permanent character independently cognizable in each individual, but more like musical notes, by their relative position on a peculiar scale which may be made to rest on any individual as an arbitrary basis.

The scale by which directions are compared is founded on the elementary relations of opposition and transverseness. In whatever direction we suppose ourselves to be traversing space, we recognize the possibility of returning to the same position from whence we set out by motion in a different direction, the relation of which to the original is that of opposition; or the two may be classed together as the positive and negative modifications of a common direction.

Again, if we fix our thoughts upon any given direction, we find a series of others in each of which it is possible to traverse space without advance in the original direction or in the one opposed to it. The directions so marked out by negation of progress in a certain direction are said to be *transverse* to the *normal* or direction in which no progress is made by the observer while advancing in the direction of any of the transverse series. If now we start afresh from any of the individuals of the latter series, it will be found that the series includes the opposite direction, as well as one direction and its opposite transverse to the former two. Every other individual of the series will be recognized as partaking in different proportions of the nature of these coordinates, or transverse directions, adopted as the basis of the scale. In other words, it will be found that distance in any intermediate direction is essentially composed of distance in the direction of each of the coordinates in different proportions, varying from all of the one and none of the other, to all of the latter and none of the former, with every modification arising from taking each of the coordinates in both a positive and a negative sense.

In like manner, as each intermediate direction is transverse to the original normal, a secondary series of directions with a different normal will arise from the combination of these coordinates in every proportion, and the whole expanse of space around the observer will be recognized as consisting of distance in every possible combination of proportions in the direction of three coordinates, of which the first may be taken at pleasure in space, the second may be identified with any of the series transverse to the first coordinate, and the third will be the single direction transverse to each of the former two. Within the sphere of three directions so related to each other we are entirely shut in. Whatever may be the particular direction in which the coordinates be laid, we can conceive no fourth direction essentially differing in nature from the former three, and therefore can conceive no possible direction which cannot be derived from some combination of three coordinates, or in which a given distance cannot be resolved into equivalent distances in the direction of the three coordinates.

We have thus in the relations of transverseness and opposition, and in the conception of intermediate directions arising from the combination of transverse coordinates in different proportions, a uniform scale by which, when applied to known directions in space, the position of any other direction may be accurately defined independent (it must be observed) of any reference to the notion of angular magnitude, of which as yet no mention has been made.

When two directions only are known in a system, they must be considered as members of the series transverse to a common normal; and one of the two being identified with the first coordinate of the scale, the position of the second will be completely determined by the proportion in which it partakes of the nature of the second coordinate or transverse direction of the series.

The directions commonly adopted as the basis of the scale, are the up and down, fore and aft, and right and left lines marked out (in any given position of the observer in a system) by the constitution of his bodily frame; and thus (in any given position of our bodies) a particular direction is defined in our thoughts by the proportion in which it partakes of the nature of those coordinates, that is to say, by the proportion in which distance in the direction in question is essentially composed of distance up or down, distance to the front or rear, and of distance to the right or left.

For the sake of simplifying the question, we will now confine our thoughts to motion in a plane surface, or to directions having reference to two transverse coordinates. Now although, in the actual apprehension of a figured system, the observer must be supposed to traverse the entire outline, and thus continually to change his place, yet he must be capable of doing so without rotation on his own axis, as he would otherwise acquire no notion of the configuration of his track in the external system. He will accordingly carry with him throughout the fundamental conceptions of front and back, right and left, and by reference to these coordinates will be able to compare and to identify directions in any part of the system.

It is in virtue of this complex scheme of relation between directions, that we are enabled to conceive the possibility of reaching the same point by different tracks from a common starting-point. We are indeed so much in the habit of thinking of points as marked out by physical phænomena (as by the letters in a geometrical illustration), that it is by no means obvious where the difficulty of the conception lies. But it must be remembered that points in geometry are distinguished solely by position, while the position of a given point is determined by the nature of the track by which it is reached from a point antecedently known. It is plain, therefore, that there would be no means of identifying points attained by tracks differing in any respect from each other, if the precise combination of distance and direction by which they were respectively attained were the ultimate test of their position. But now the knowledge of the fundamental scheme of relationship above explained makes us regard the space traversed in each successive instant of time in the track by which the position of a point is determined (and consequently the whole space traversed in the entire track), as equivalent to a certain distance in the direction of each of the two coordinates of the scale. The aggregate character (in respect of distance and direction) of the space traversed in different tracks (by which the position of the terminal points is governed) will thus be made to depend on the aggregate distance advanced in the direction of the two coordinates, a question to be tried by simple superposition. When the distance advanced in the direction of each coordinate is the same, the positions finally attained will be recognized as identical, and the points will coincide whatever may be the amount of intermediate divergence in the tracks by which they have actually been reached.

From the same principle it may be shown, that a straight line may be drawn from a given point to any other point in space. Because the space traversed in the track by which the second point must be supposed to have been determined, will be equivalent in distance and direction to a certain distance in each of the two standard directions of the system. Now inasmuch as the series of directions intermediate between any pair of transverse directions includes individuals partaking in every conceivable proportion of the nature of both the transverse directions between which they lie, it will always be possible to select one of the series a certain distance in which will be equivalent to given distances in each of the two transverse directions, and therefore the distances in the direction of the coordinates of the system under consideration, into which the space traversed in the original track has been resolved, may again be exchanged for an equivalent distance in a single direction duly related to each of the coordinates; in other words, the same position may be attained by motion in a single continuous direction as by a track of any other description, or what amounts to the same thing, a straight line may be drawn from a given point to a point determined by a track of any other description.

As soon as a straight line is known as lying in a single continuous

direction, it becomes the most obvious means of marking the direction so exhibited throughout a finite extent of line. The series of directions transverse to a given normal may then be represented by two straight lines crossing each other at right angles, and an indefinite number of other straight lines diverging from the point of intersection, and dividing the plane surface round that point into as many parts as there are diverging lines. If now we take two of these lines, like the hands of a clock, and suppose one to remain fixed while the other revolves from left to right, it will pass successively through all the directions intermediate between left and front, while the quantity of plane surface intercepted between the hands abutting on the point of intersection will continually increase as the difference in their direction becomes greater, or in proportion as distance in the direction of the moveable hand contains a greater proportion of distance in the direction transverse to that of the fixed one. Thus we are taught a new mode of estimating the relation between the direction of straight lines diverging from a common point; not by a proportion which addresses itself to the understanding merely, but by a quantity admitting of measurement by bodily comparison, viz. by the quantity of plane surface intercepted between the diverging lines and abutting on the point of intersection, or by the magnitude of the included angle.

Professor Challis gave an account of a luminous appearance observed at the time of the perihelion passage of Klinkerfue's comet.

Professor Stokes read a paper on the Optical properties of Light reflected from Crystals of Permanganate of Potash. The substance of this paper is embodied in a paper on the Metallic Reflexion exhibited by certain Non-metallic Bodies, published in the Philosophical Magazine, vol. vi. p. 393.

December 12, 1853.

Professor Fisher read the first part of a paper, entitled "Researches, Physiological and Pathological, on the Development of the Vertebral System."

After having explained what he meant by the term *vertebral system*, he stated (and he illustrated what he described by drawings) that the spinal marrow, at a particular stage of growth of the human embryon, exhibits indications of segmental development corresponding to that of the spinal column; that is to say, that each of its halves offers on its external surface a series of symmetrical spaces defined by transverse lines, each of which spaces corresponds to the roots of a single spinal nerve; and again, that each half presents in its internal structure, a double series, one anterior, the other posterior, of symmetrical areas, two of which appeared to equal in extent one of the external spaces just spoken of. Professor Fisher also

stated that the spinal marrow offers, at the period of development in question, several other peculiarities, some of them bearing likewise a segmental character; but he reserved a detailed description of them for a future communication.

February 27, 1854.

A paper was read by Professor Challis, entitled "A direct Method of obtaining by Analysis the mean motions of the apse and node of the Moon's Orbit." See Philosophical Magazine, vol. vii. p. 278.

Also a paper by Mr. J. B. Phear on some parts of the Geology of Suffolk, particularly with reference to the Valley of the Gipping.

The deposits which constitute what is often termed the glacial formation, but which the present state of our knowledge hardly allows us to designate by a name significant of a common origin, present so much confusion to the inquirer, and impose upon him so much laborious research by the extent and the unconnected character of their distribution, that they have hitherto met with less attention than their importance deserves.

The county of Suffolk seems to be a district where a portion of these deposits is manifested with more than usual distinctness, and is capable of being studied with comparative facility. The county is separated from Norfolk on the north by the well-marked valleys of the Ouse and the Waveney, is bounded on the east and south by the sea and valley of the Stour, and is bordered by chalk uplands on the north-west; the whole central portion is thickly covered with a mass of blue drift-clay, cut into abrupt undulations by a network of valleys. This clay is totally without any symptom of stratification, and is full of fragments of all rocks of the secondary period, including specimens of granite and other igneous rocks.

Wells sunk in different parts of the county show this drift-clay to have a thickness varying from 200 feet to a few inches; it seems to thin off from the northern and western parts of the county towards the coast, and only exists in the shape of outliers beyond a line passing through Sudbury, Hadleigh, Bramford, Woodbridge, and Saxmundham; a line, it may be remarked, nearly coinciding with the edge both of the London clay and of the crag, and approximately passing through the heads of the tidal estuaries of the Orwell, Deben, Ore and Alde. The clay is almost universally underlaid by an unfossiliferous sand; and there is reason to conjecture that this sand, of a prevailing red colour, passes out beyond the just-mentioned line, and covers in many places the surface of the strip of land between it and the sea.

A detailed examination of the Gipping valley reveals a well-marked and connected line of sand cliffs fringing it, and its Coddenham tributary in particular, at a high level on both sides; the sand is generally pure white, though often red, horizontally stratified and

capped with an unrolled gravel, which evidently owed its existence to the quiet washing away of the drift-clay from its insoluble contents. Above Needham Market the valley is channelled in drift-clay, but between Needham and Bramford it is cut through chalk; and it should be remarked, that the line of sand-hills does not extend up the valley with any great distinctness beyond the chalk. The phænomena seen at Creeting are not consistent with this sand lying beneath the drift-clay; and the inference is, that it constitutes the remains of an estuary deposit formed in the valley subsequent to its excavation in the drift-clay.

All the other streams west of the Gipping have chalk for their floor during the middle part of their course, thus manifesting the existence of a ridge of chalk running beneath the drift accumulations nearly due west and east from Sudbury to Bramford. Disturbances evidenced in this ridge, and perhaps due to its elevation, are partaken of by the London clay and crag deposits which overlie it on the east and south.

In Norfolk the drift-clay attains a greater thickness than in Suffolk, and towards the north of the county is overlaid by a sand and gravel formation which may be appropriately termed upper drift. The gradual disappearance of this towards the south, together with the thinning away and final extinction of the drift-clay in the same direction, point to a region of greater denuding activity; it may be an interesting question whether such denudation be in any degree connected with the upheaval of the before-mentioned chalk ridge, or again, whether the sands of the Gipping valley bear any relation to the upper drift of Norfolk.

March 13, 1854.

A paper was read by Prof. Challis on the Eccentricity of the Moon's Orbit; supplement to a former communication on the mean motions of the Apse and Node. See the former paper, Phil. Mag. vol. vii. p. 278.

Also a paper by Mr. J. Clerk Maxwell on the Transformation of Surfaces by Bending.

The kind of transformation here considered is that in which a surface changes its form without extension or contraction of any of its parts. Such a process may be called bending or development. The most obvious case is that in which the surface is originally a plane, and becomes, by bending, one of the class called "developable surfaces." Surfaces generated by straight lines, which do not ultimately intersect, may also be bent about these straight lines as axes. In this way they may be transformed into surfaces whose generating lines are parallel to a given plane, just as the former class are transformed into planes.

In both these cases, the bending round one straight line of the

system is quite independent of that round any other; but in those which follow, the bending at one point influences that at every other point. The case of a surface of revolution bent symmetrically with respect to the axis is taken as an example.

The remainder of the paper contains an elementary investigation of the conditions of bending of a surface of any form.

The surface is considered as the limit of the inscribed polyhedron when the number of the sides is increased and their size diminished indefinitely.

A method is then given by which a polyhedron with triangular facets may be inscribed in any surface; and it is shown, that when a certain condition is fulfilled, the triangles unite in pairs so as to form a polyhedron with quadrilateral facets. The edges of this polyhedron form two intersecting systems of polygons, which become in the limit curves of double curvature; and when the condition referred to is satisfied, the two systems of curves are said to be "conjugate" to one another.

The solid angle formed by four facets which meet in a point is then considered, and in this way a "measure of curvature" of the surface at that point is obtained.

It is then shown that if there be two surfaces, one of which has been developed from the other, one, and only one, pair of systems of corresponding lines can be drawn on the two surfaces so as to be conjugate to each other on both surfaces. This pair of systems completely determines the nature of the transformation, and is called a double system of "lines of bending." By means of these lines the most general cases are reduced to that of the quadrilateral polyhedron. The condition to be fulfilled at every point of the surface during bending is deduced from the consideration of one solid angle of the polyhedron. It is found that the product of the principal radii of curvature is constant.

By considering the angles of the four edges which meet in a point, we obtain certain conditions, which must be satisfied by the lines of bending in order that any bending may be possible. If one of these conditions be satisfied, an infinitesimal amount of bending may take place, after which the system of lines must be altered that the bending may continue. Such lines of bending are in continual motion over the surface during bending, and may be called "instantaneous lines of bending." When a second condition is satisfied, a finite amount of bending may take place about the same system of lines. Such a system may be called a "permanent system of lines of bending."

Every conception required by the problem is thus rendered perfectly definite and intelligible, and the difficulties of further investigation are entirely analytical. No attempt has been made to overcome these, as the elementary considerations previously employed would soon become too complicated to be of any use.

For the analytical treatment of the subject the reader is referred to the following memoirs:—

1. "Disquisitiones generales circa superficies curvas," by M. C.

F. Gauss (1827).—*Comm. Recentiores Gott.* vol. vi.; and in Monge's "Application de l'Analyse à la Géométrie," edit. 1850.

2. "Sur un Théorème de M. Gauss, &c.," par J. Liouville.—*Liouville's Journal*, 1847.

3. "Démonstration d'un Théorème de M. Gauss," par M. J. Bertrand.—*Liouville's Journal*, 1848.

4. "Démonstration d'un Théorème," Note de M. Diguet.—*Liouville's Journal*, 1848.

5. "Sur le même Théorème," par M. Puiseux.—*Liouville's Journal*, 1848.

And two notes appended by M. Liouville to his edition of Monge.

March 28, 1854.

Prof. Miller gave an account of the relation between the physical characters and form of crystals of the oblique system as established by the observations of Mitscherlich, Neumann, De Senarmont, Wiedemann and Ångström.

A paper was read by Prof. De Morgan on some Points in the theory of differential equations.

1. The words *primordinal*, *biordinal*, &c. are used in abbreviation of the phrases 'of the first order,' 'of the second order,' &c.

The symbol for a differential coefficient, U_x for $\frac{dU}{dx}$, &c., long used by the author, is thus extended. By $U_{x|p,q}$ is meant $dU:dx$ with reference to x as contained in p and q, as well as explicitly. Thus $U_{x|p,q}$ means $U_x+U_p p_x+U_q q_x$; and $U_{x|y}$ means $U_x+U_y y'$.

Differentiations are sometimes expressed thus: $d_x U=U_x\,dx$, $d_{x,y}U=U_x dx+U_y dy$.

When it is only requisite to express functional relation, without specification of form, $(x, y, z)=0$ or $z=(x, y)$ may signify an equation between x, y, and z. A letter may be used as its own functional symbol: thus $u=u(x, y, z)$ may signify that u is a function of x, y, z. And in 'for u write $u(x, y, z)$' there is a convenient abbreviation of 'for u substitute its value in terms of x, y, z.'

2. When, as so often happens, a variable enters under relations which destroy the effect of its variation upon the *form* of differential coefficients, it is called *self-compensating*. Thus $\phi(x, y, a)=0$, $\phi_a(x, y, a)=0$, contain the self-compensating variable a. Similarly, when $\phi(x, y, a, b)=0$ is accompanied by $\phi_a da+\phi_b db=0$, a and b are mutually compensative, and primordinally. The addition of

$$\phi_{a(x|y)}\,da+\phi_{b(x|y)}\,db=0$$

makes a and b biordinally compensative.

3. When a finite change in x makes an infinite change in y, it makes an infinite change in $y':y$, in $y'':y'$, &c. When *either* or

both P and Q become infinite, $P : Q$ and $P_x : Q_x$ are both nothing, both finite and equal, or both infinite; provided that the infinite form is produced by *substitution for x*. If $u=(v, w, \ldots)$, any relation which makes u_v infinite either makes u_w infinite, or is independent of w. And if $u_v=\infty$ be produced by a relation containing v, then $u_v dv+u_w dw+\ldots=0$ and $u_{vw} dv+u_{vw} dv+\ldots=0$ are relations of identical meaning.

4. From the last it follows that $U=\text{const.}$ is solved by making any factor of dU either 0 or ∞. In $dU=M(Pdx+Qdy)$, singular solutions are obtained, as is known, from $M=\infty$: it ought to be asked whether $M=0$ does not give singular *exceptions*, that is, cases in which $U=\text{const.}$ arises otherwise than from $P+Qy'=0$. It is found more convenient to treat these cases without actual separation of the factor; that is, from $dU=U_x dx+U_y dy$.

5. In a former paper, the author insisted on the arbitrary functions which enter the intermediate primitives: maintaining, for example, that the primordinal of $y''=0$ is $\phi(y', xy'-y)=0$, for any form of ϕ. Lagrange, he has since found, notices this extension, and rejects it, because it leads to $y'=a$, $xy'-y=b$, as necessary consequences of its ordinary solution. Mr. De Morgan maintains his opinion, and observes that Lagrange's reason would make it imperative to reject one of the two, $y'=a$, $xy'-y=b$, since either is the necessary consequence of the other.

6. In order to avoid the ambiguous use of the word *singular*, a singular solution is defined as any one which, by the mode of obtaining it, cannot have the ordinal number of constants: it is further styled *intraneous* or *extraneous*, according as it is or is not a case of the general solution. If $y=\psi(x, a)$ or $a=A(x, y)$ give $y'=\chi(x, y)$, then $dA=A_y(y'-\chi)dx$ and $\chi=-A_x : A_y$ are identical equations. Every relation which satisfies $A_y=\infty$ *is* a solution, and a singular solution; except possibly, relations of the form $x=\text{const.}$, which must always be examined apart. Also, $A_y=\infty$ is identical with $\psi_a=0$. There can exist no solutions whatsoever except those which are contained in $A=\text{const.}$, $A_y=\infty$, and (possibly) $x=\text{const.}$

Again, $\chi_y=(\log \psi_a)_x$. Of this equation the author has found neither notice nor use: supposing it to have ever been given, he holds it most remarkable that it has not become common as the mode of connecting the two well-known and widely used tests of singular solution. It easily shows that $\chi_y=\infty$ contains all extraneous solutions, and all intraneous solutions which (as often happens) can be also obtained by making a a function of x. It also easily gives a conclusion arrived at by the author in his last paper, namely, that when $\chi_y=\infty$ is satisfied and not $y'=\chi$, it follows that $\chi_x+\chi_y\chi$ is infinite.

7. The author gives his own version of the demonstration of a theorem of M. Cauchy, for distinguishing extraneous and intraneous solutions. If $y=P$, P being a given function of x, satisfy $y'=\chi(x, y)$, that is, if P' and $\chi(x, P)$ be identical, then $y=P$ is an extraneous or

intraneous solution of $y'=\chi(x, y)$, according as

$$\int_{P}^{P+\beta} \frac{dy}{\chi(x,y)-\chi(x,P)}$$

(x being constant) is finite or infinite for small values of β. This theorem has attracted little notice in this country: the author believes it to be fully demonstrated, and considers it one of the most remarkable accessions of this century to the theory of differential equations.

8. It is observed that the validity of the extraneous solution may depend upon the interpretation of the sign of equality by which $A=B$ is held satisfied when both sides are 0, or both infinite, even though $A:B=1$ is not satisfied. Thus $y'=2\sqrt{y}$ or $y=(x+a)^2$, has the extraneous solution $y=0$, which, however, is not a solution if by $y'=2\sqrt{y}$ we understand in *all* cases $y' : \sqrt{y}=2$.

9. The common mode of obtaining the singular solution of a biordinal (by combining $\phi(x, y, a, b)=0$, $d_{a,b}\phi=0$, $d_{a,b}\phi_{x|y}=0$) though sufficiently general, is never *shown* to be so.

Let $y=\psi(x, a, b)$, combined with $y'=\psi_x$, give $a=A(x, y, y')$, $b=B(x, y, y')$, from either of which follows $y''=\chi(x, y, y')$. The most general primordinal is $f(A, B)=0$, f being arbitrary. Any given curve, $y=\varpi x$, may be made to solve this for some form of f; but, generally speaking, this solution will be *extraneous*. For A and B are so related that every intraneous solution makes A and B constant. And any primordinal equation whatever may in an infinite number of ways be thrown into the form $f(A, B)=0$, so that the intraneous solutions shall make A and B constant.

(Given $y=\varpi x$, required a key to all the primordinals of which it is a singular solution. Take *any* equation $y=\psi(x, a, b)$, eliminate x between $a=A(x, \varpi x, \varpi' x)$ and $b=B(x, \varpi x, \varpi' x)$, and write $A(x, y, y')$ and $B(x, y, y')$ for a and b in the result.)

The equations $dA=A_{y'}(y''-\chi)dx$, $dB=B_{y'}(y''-\chi)dx$ are identically true. And $A_{y'}=\infty$, or any relation which satisfies it, is a singular primordinal of $y''=\chi$, whenever it is a primordinal at all; that is, when y' appears in it. When $A_{y'}=\infty$ is satisfied by a relation void of y', that relation is not necessarily a solution. The ordinary solutions of $A_{y'}=\infty$ are solutions of $y''=\chi$; but not (necessarily) the singular solutions. The singular solutions of a relation which makes $A_{y'}=\infty$ may make $A_{y'}$ finite.

Comparing A and B with ψ, we have

$$A_{y'}=-\frac{\psi_b}{\psi_a\psi_{bx}-\psi_b\psi_{ax}}, \quad B_{y'}=\frac{\psi_a}{\psi_a\psi_{bx}-\psi_b\psi_{ax}},$$

$$\chi_{y'}=\{\log(\psi_a\psi_{bx}-\psi_b\psi_{ax})\}_x.$$

From these are obtained results in complete analogy with those for primordinal equations. But when $\psi_a\psi_{bx}-\psi_b\psi_{ax}=0$, the usual criterion of singular solution, is made valid by $\psi_a=0$, $\psi_b=0$, a singular primitive of the singular primordinal may be obtained, which does not necessarily satisfy $y''=\chi$.

10. Similar forms are given for *triordinal* equations. In noticing the manner in which the equations of the general theory may be easily expressed by what are called *determinants*, Mr. De Morgan expresses his admiration of the system, and his sense of the important services rendered by those who have laid its foundations. But he refuses to employ the word *determinant* in the sense proposed, on account of its not expressing any *distinctive* property of these functions. Until those who have a better right to give a name provide themselves with a *distinctive* one, he intends to call them *eliminants*.

The forms connected with $y=\psi(x, a, b)$ may be easily translated into others derived from $\phi(x, y, a, b)=0$. But the formula which connects $\chi_{y'}$ with ϕ is as follows:—

$$\chi_{y'}=\left\{\log\left(\frac{\phi_a\phi_{bx|y}-\phi_b\phi_{ax|y}}{\phi_y^2}\right)\right\}_{x|y}+\frac{\phi_a\phi_{by}-\phi_b\phi_{ay}}{\phi_a\phi_{bx|y}-\phi_b\phi_{ax|y}}\cdot\chi,$$

where by $U_{x|y}$ is meant $U_x+U_y y'$, even when U is a function of y'. Thus $(xy'-y)_{x|y}$, as here used, is 0.

11. The following idea of reciprocal polarity has been presented by M. Druckenmüller (as cited from Crelle's Journal by Mr. Boole), and, independently, by Professor Boole: it occurred to the author of this paper before he had seen the researches of either. If there be equations involving $m+n$ variables, and if, determining a point by fixing m of the variables, a curve be determined by giving all possible values to the remaining n (*point* and *curve* being here merely names of objects determined), we may say that the (m)-point is the pole of the (n)-curve. Similarly, we may make each (n)-point the pole of an (m)-curve. And all the points of any curve have polar curves which contain the pole of that curve. If the two sets of variables be severally made primordinally compensative, the general properties which arise are easy extensions of the well-known theory of reciprocal polars. Let (x, y) and (a, b) be two points: the polar property of $x^2+y^2=ax+by$ contains the direct and converse property of the angle in a semicircle. If $\phi(x, y, a, b)$ be the *modular equation*, and if x, y and a, b be compensative, any element (x, y, y') of *any* (x, y)-curve to the pole (a, b) determines an element (a, b, b') of an (a, b)-curve to the pole (x, y). These curves are *reciprocal polars*. In the common system, the modular equation is linear with respect to both pairs of coordinates, and the locus of those poles which lie in their polar straight lines is a conic section, to which the polars are tangents.

12. The method of transforming differential equations, given by the author in his last paper, is precisely the reference of the curves sought to their reciprocal polars, the modular equation being taken at pleasure. Mr. De Morgan now proposes to call it the method of *polar transformation*. Let $\phi(x, y, a, b)=0$ be the modular equation, and let $\phi_x+\phi_y y'=0$, $\phi_a+\phi_b b'=0$, b' being $db : da$. Hence

$$a=\mathrm{A}(x, y, y'),\ b=\mathrm{B}(x, y, y');\quad x=\mathrm{X}(a, b, b'),\ y=\mathrm{Y}(a, b, b')$$
$$b'=\mathrm{B}_{y'}\div\mathrm{A}_{y'}\qquad\qquad y'=\mathrm{Y}_{b'}\div\mathrm{X}_{b'};$$

the biordinal factors, $y''-\chi(x, y, y')$, $b''-\alpha(a, b, b')$, disappearing

from b' and y'. Hence b' depends on x, y, y'. Similarly, b'' depends on x, y, y', y'', &c., and similarly for y', y'', &c. If in $f(x, y, y', y'', \text{\&c.})=0$ we substitute for x, y, y', &c. in terms of a, b, b', &c., the two equations belong to polar reciprocals. If either can be integrated, the integration of the other depends on elimination: thus if the equation in a, b, &c. can be integrated, the solution of the equation in x, y is obtained by eliminating a and b between the integral obtained and $x=\mathrm{X}$, $y=\mathrm{Y}$.

13. There are two reciprocal biordinal equations belonging to the modular equation $\phi(x, y, a, b)=0$; $y''=\chi$ when a and b are constant, $b''=\alpha$ when x and y are constant. The two have the same condition of singular solution; for $\mathrm{A}_{y'}\phi_b=\mathrm{X}_{b'}\phi_y$. Let this be $\sigma(x, y, a, b)=0$, when cleared of y' or b'. The following table exhibits the relations of the double system:—

$$\begin{array}{cccccccc}
 & & & \multicolumn{2}{c}{\phi(x,y,a,b)=0} & & & \\
\sigma(x,y,a,b)=0 & \multicolumn{2}{c}{\phi_x+\phi_y y'=0} & & & \multicolumn{2}{c}{\phi_a+\phi_b b'=0} & \sigma(x,y,a,b)=0 \\
y'=\varpi(x,y) & a=\mathrm{A}(x,y,y') & b=\mathrm{B}(x,y,y') & & & x=\mathrm{X}(a,b,b') & y=\mathrm{Y}(a,b,b') & b'=\lambda(a,b) \\
y=\Pi(x,\mathrm{C}) & \multicolumn{2}{c}{y''=\chi(x,y,y')} & & & \multicolumn{2}{c}{b''=\alpha(a,b,b')} & b=\Lambda(a,\mathrm{Z}).
\end{array}$$

Eliminate a and b between $\phi=0$, $\sigma=0$, $\phi_{x|y}=0$, and we have $y'=\varpi$, $y=\Pi$, the singular primordinal and primitive of $y''=\chi$; those of $b''=\alpha$ are obtained by eliminating x and y from $\phi=0$, $\sigma=0$, $\phi_{x|y}=0$. There is a relation involved between C and Z, the constants of integration. For each value of C, $y=\Pi$ is the xy-curve which touches all in $\phi(x, y, a, \Lambda)=0$, for the corresponding value of Z and all values of a. The same of Z, $b=\Lambda$, and $\phi(x, \Pi, a, b)=0$. The contacts are of the second order, and $y=\Pi$, $b=\Lambda$, are polar reciprocals for corresponding values of C and Z. But the singular primitives of $y'=\varpi$ and $b'=\lambda$ are not necessarily reciprocals: when this does happen, their contacts with primitives are of the third order.

14. When a surface is described by one set of curves, as in the surface obtained by eliminating a from $\phi(x,y,z,a)=0$, $\psi(x,y,z,a)=0$, it is proposed to call it a *shaded* surface, and the curves *lines of shading*. The equation $f(x, y, z, y', z')=0$, y and z being functions of x, cannot, generally, belong to any family of surfaces in an unrestricted sense; that is, it cannot be always true of a point moving in any way upon a surface. Such a supposition would be equivalent to imagining a surface *every* point of which has the primordinal character of the vertex of a cone. But it may belong to any surface, properly shaded, or to any mode of shading, if the proper surface be chosen.

15. Two equations of the form $y=\Phi(x, a, b, c)$ $z=\Psi(x, a, b, c)$, give one, and only one, primordinal of the form $f(x, y, z, y', z')=0$. Assume any surface $\omega(x, y, z)=0$; by this, and compensative relations between a, b, c, another pair of primitives may be found. But the primitives obtained from $\omega=0$ do not *shade* this surface, except in cases determined by two relations between the constants. Again, making a, b, c compensative, without any assumed surface, we find

one equation of the form $(a, b, c, a', b')=0$, any primitives of which lead to other primitive forms for $f=0$. Each of the second primitives has contact of the first order with one family of curves from among the original primitives; and all ordinary primitives are found, in an infinite number of ways, among the connecting curves of others. There is a singular solution, a curve of contact to all primitives, when $\Phi_a=0$, $\Psi_a=0$, &c. can all be satisfied at once.

Since $y=\Phi$, $z=\Psi$, give a primordinal equation independent of constants, the polar reciprocal properties of curves in space are of a restricted form. Every surface dictates another surface, and a mode of shading *both*, so that each line of shading on either surface is the polar reciprocal of a line on the other.

16. The conversion of constants into compensative variables may give restricted solutions, as in the ordinary case of two variables, and every other in which the constants are converted into separately self-compensating variables. When these variables are made collectively compensating, and the equations permit elimination of the original variables, ordinary differential equations may be produced, the integration of which may, after substitution, give primitives of the same form as those from which they came. But when the original variables cannot be eliminated, arbitrary relations may be required, in number enough to eliminate the differentials of the new variables: in this case arbitrary functions enter the primitives finally deduced. Of this last case one instance is Lagrange's transition from a primitive of a primordinal partial equation having two constants to the complete primitive of that equation.

17. A biordinal partial equation may be produced from

$$U(x, y, z, a, b, c, e, h)=0$$

by eliminating the five constants between $U=0$ and the five results of primordinal and biordinal differentiation. But it is not true that every form of $U=0$ leads to one biordinal equation only: many forms lead to an infinite number. Two attempts to procure other primitives by making a, b, &c. compensative variables, end in two different forms of result. First, when all the resulting equations are required to be integrable, by introduction of a proper factor, the success of the process requires the integral of two partial equations, one primordinal and one biordinal, between four variables. Secondly, when no such condition is required, the result is another form involving five constants.

18. A primordinal partial equation belongs to a family of surfaces of which one is determined by any given curve through which it is to be drawn. A biordinal equation belongs to an infinite number of families; and a distinct conception of the conditions which select an individual surface is best formed by an extension of the following kind. A curve on a surface is analogous to a point on a curve: two curves being drawn on a surface, the analogue of the chord joining two points on a curve is the developable surface (or surfaces) drawn through the two curves. The developable surface which touches the given surface in a curve (and not the tangent plane) is the ana-

logue of the line which touches a curve in a point. A biordinal equation being given, one surface satisfying it is selected by a curve through which that surface is to pass, and a developable surface passing through that curve which the surface is to touch.

19, 20. The restrictions under which two arbitrary forms must enter, in order that a biordinal partial equation may exist independent of these functions, are wholly unknown. The case which is fully analogous to a biordinal of two variables, is of the most limited character. Ampère has noticed this: Mr. De Morgan was led to it by an examination of the polar properties of $\phi(x, y, z, a, b, c)=0$. This equation leads to $a=\mathrm{A}$, $b=\mathrm{B}$, $c=\mathrm{C}$, where A, B, C are functions of x, y, z, p, q. The primordinal $f(\mathrm{A}, \mathrm{B}, \mathrm{C})=0$ is satisfied by $\phi=0$, subject to $\phi(a, b, c)=0$, and leads to a biordinal, independent of f, of the form

$$\mathrm{Q}+\mathrm{R}r+\mathrm{S}s+\mathrm{T}t+\mathrm{U}(s^2-rt)=0,$$

in which Q, R, &c. are not wholly independent of each other.

If the pole (a, b, c) move along a certain curve, the polar surface must touch a certain surface in one of the lines of a certain shading. That is, every *abc*-curve has a shaded surface, which is its polar reciprocal: and every line of shading of that surface has another surface for its polar reciprocal, shaded by lines of which the original *abc*-curve is one. And every surface has a reciprocal surface such that for each point on one there is a point on the other; and the point on one surface being taken, the polar surface of that point touches the other surface in the other point.

The singular solutions of the two biordinals derived from

$$\phi(x, y, z, a, b, c)=0$$

by means of x, y, z and of a, b, c, are connected by relations analogous to those already seen in the case of two variables. In fact, there is perfect coincidence and coextension between the properties of the general equation $y''=\chi(x, y, y')$ and a particular species of the equation $\mathrm{Q}+\mathrm{R}r+\mathrm{S}s+\mathrm{T}t+\mathrm{U}(s^2-rt)=0$. It is proposed to call this species the *polar biordinal*.

21. The general method of transforming partial equations, given in the last paper, is the investigation of the class of surfaces contained under a given equation by reference of them to their polar reciprocals, any convenient modular equation $\phi(x, y, z, a, b, c)$ being made the means of transformation.

22. The following notation is proposed for eliminants. The components being A_p, A_q, &c., B_p, &c., the eliminants are $(\mathrm{A}_{p)}$, $(\mathrm{AB}_{pq)}$, $(\mathrm{ABC}_{pqr)}$, &c.; the components being A, A′, &c. B, &c., the eliminants are (A°), $(\mathrm{AB}^{\circ\,\prime})$, $(\mathrm{ABC}^{\circ\,\prime\,\prime\prime})$, &c. Thus

$$(\mathrm{A}_{p)}=\mathrm{A}_p$$

$$(\mathrm{AB}_{pq)}=\mathrm{A}_p(\mathrm{B}_{q)})-\mathrm{B}_p(\mathrm{A}_{q)})$$

$$(\mathrm{ABC}_{pqr)}=\mathrm{A}_p(\mathrm{BC}_{qr)})+\mathrm{B}_p(\mathrm{CA}_{qr)})+\mathrm{C}_p(\mathrm{AB}_{qr)})$$

$$(\mathrm{ABCD}_{pqrs)}=\mathrm{A}_p(\mathrm{BCD}_{qrs)})-\mathrm{B}_p(\mathrm{CDA}_{qrs)})+\mathrm{C}_p(\mathrm{DAB}_{qrs)})-\mathrm{D}_p(\mathrm{ABC}_{qrs)}),$$

and so on. Some slight investigation of properties is made, to exhibit the notation.

The following rule is suggested to determine, in any complicated case, whether the number of contiguous interchanges by which one arrangement of letters is converted into another shall be odd or even. This is an important matter in the theory of eliminants, though very complicated instances may seldom occur in practice. Write down one arrangement under the other, and, beginning at one letter in one line, mark the companion letter in the other line, pass on to that companion in the first line, mark its companion, and so on, until we arrive at a letter already marked. Call this sequence a *chain*, each mark being one *link*. Having formed one chain, begin at a letter not yet used, and form another; and so on until every letter has been used. Then, according as the number of chains with *even* links is odd or even, the number of interchanges of contiguous letters required is odd or even. For example, the two arrangements being

A B C D E F G H I J K L M N O P Q

H M O G Q B K L J P F C I N A D E

1 2 1 2 3 2 2 1 2 2 2 1 2 4 1 2 3.

Under A is H, under H is L, under L is C, under C is O, under O is A, already taken: the first chain has five links, the second is found to have nine, the third two, the fourth one. The number having even links is one, an odd number; hence an odd number of contiguous interchanges converts the first arrangement into the second.

23. The following is the method of ascertaining whether the biordinal equation

$$Q+Rr+Ss+Tt+U(s^2-rt)=0 \quad . \quad . \quad . \quad . \quad (1)$$

possesses a primordinal of the form $f(x, y, z, p, q)=0$, containing an arbitrary function. Considering x, y, z, p, q as five independent variables, integrate, by common methods, the equations

$$U\left(\frac{dv}{dx}+p\frac{dv}{dz}\right)+T\frac{dv}{dp}-\frac{k}{1+k}S\frac{dv}{dq}=0$$

$$U\left(\frac{dv}{dy}+q\frac{dv}{dz}\right)+R\frac{dv}{dq}-\frac{1}{1+k}S\frac{dv}{dp}=0,$$

k being one of the roots of $kS^2=(1+k)^2(RT+QU)$. If a common solution $v=A$ can be found, then $A=$const. is a primordinal of (1).

If two common solutions, A and B, can be found, then $B=\varpi A$ is a primordinal, ϖ being arbitrary. But though in this case $A=$const. and $B=$const. are solutions, they cannot *coexist*, unless the values of k be equal, or unless $S^2=4(RT+QU)$. This last equation is one condition of polarity; and if, when satisfied, we find *three* (and there cannot be more) common solutions, A, B, C, inexpressible in terms of each other, then $f(A, B, C)=0$ is the most general primordinal, any two forms of it may coexist, or even any three, which amount to $A=$const., $B=$const., $C=$const. Elimination of p and q between these last equations gives $\phi(x, y, z, a, b, c)=0$, the modular equa-

tion. And the general solution of (1) is found by assuming b and c in terms of a, and then making a a self-compensating variable.

24. The paper is concluded by some remarks on notation.

In an appendix to the preceding paper, read to the Society on the 1st of May, 1854, Mr. De Morgan points out an error committed by M. Cauchy in a very remarkable theorem, of which his enunciation is as follows.

Let ϕx be a function which can be developed in integer powers of x. Let $r(\cos\theta+\sin\theta\,.\,\sqrt{-1})$, r being positive, be any one of the roots of $\phi x=\infty$ or of $\phi' x=\infty$. Then the development of ϕx is convergent from $x=0$ up to $x =$ the least value of r.

M. Cauchy stipulates that the function shall be continuous; but he defines a function to be continuous so long as it remains finite, and receives only infinitely small increments from infinitely small accessions to the variable. It is then obviously impossible that the above theorem should be universally true. Were it so, it would follow that the development of $(1+x)^{\frac{3}{2}}$ is convergent for all finite values of x, whereas it is well known that this development becomes divergent when x is greater than unity. The error of M. Cauchy's demonstration (which contains a valuable method for establishing a large class of definite integrals) is the assumption that if an infinite number of convergent series of the form $a+bx+cx^2+\ldots$, all with one value of x but different values of $a, b, c, \ldots$, be added together, the sum divided by the number of series is also a convergent series. This assumption is not universally true.

Mr. De Morgan takes a totally different line of demonstration, and establishes the following theorems.

If $r(\cos\theta+\sin\theta\,.\,\sqrt{-1})$, r being positive, represent a root of any one of the equations $\phi x=\infty$, $\phi' x=\infty$, $\phi'' x=\infty$, ... then the development of ϕx in powers of x is always convergent from $x=0$ up to $x=$ the least value of r, and divergent for all greater values of x.

If the development have all its coefficients positive, or if all beyond an assignable coefficient be positive, the least value of r is obtained from a real and positive root.

If the signs of the development be, or finally become, recurring cycles, with l in each cycle, the least value of r is obtained from a root in which $\cos\theta+\sin\theta\,.\,\sqrt{-1}$ is one of the lth roots of unity. If no such cycle be finally established, $\cos\theta+\sin\theta\,.\,\sqrt{-1}$ may have a value of θ which is incommensurable with the right angle.

M. Cauchy has established from his own theorem (the want of sufficient statement of conditions not affecting this particular case) the necessity of the observed fact, that the developments produced by Lagrange's theorem for the development of implied functions always give, when convergent, the least of the real values which are implied.

May 1, 1854.

A paper was read by Professor De Morgan on the Convergency of Maclaurin's Series, being an Appendix to a paper on some Points in the theory of differential equations. See the abstract of the former paper, Phil. Mag. vol. vii. p. 450.

Mr. Kingsley made an oral communication on the Chemical Nature of Photographic Processes.

May 15, 1854.

A paper was read by Mr. Warburton on Self-repeating Series.

In computing Bernoulli numbers by the formula of Laplace*, the author of this paper was led to notice, that in the fraction whose development is a series of the form $1^{2n+1}-2^{2n+1}t+3^{2n+1}.t^2-$&c., the numerator of that fraction is a recurrent function of t. This led him to investigate the question, what are the conditions which the denominator of the generating fraction, and the terms of the series generated, must satisfy, in order that the numerator of such a fraction may be a recurrent function of t. The paper contains the result of that investigation.

The author calls those series "*self-repeating*," which, when extended without limit in opposite directions, admit of separation into two similar arms, each arm beginning with a finite term of the same magnitude. Between this pair of finite terms, either no zero-term, or one or more zero-terms, may intervene. One arm repeats, and contains arranged in reverse order, the terms of the other arm, either all, or none, of the terms having their signs changed. The different positive integer powers of the natural numbers, of the odd numbers, and of the figurate numbers of the several orders, present familiar examples of self-repeating recurring series.

The author demonstrates the following three theorems respecting self-repeating recurring series:—

I. If the series arising from the development of a proper fraction is the right arm of a self-repeating recurring series, and if the denominator of such a fraction is a recurrent function of t, then the numerator also is a recurrent function of t.

II. Other things remaining the same, if the numerator of the fraction is a recurrent function of t, then the denominator also is a recurrent function of t.

III. If the numerator and the denominator of a proper fraction are each a recurrent function of t, then the series, arising from the development of the fraction according to the positive integer powers of t, will be the right arm of a self-repeating recurring series.

By way of example, the author applies his first theorem to the summation of the infinite series $1^9-2^9+3^9-$&c., and compares his process with the corresponding processes of Laplace and of Sir John Herschel. The sum in question is given by Sir John Herschel (see Jameson's Journal, January 1820) in terms of the differences of the powers of 0, extending from $\Delta^1 0^9$ to $\Delta^9 0^9$. In the author's process,

* See Memoirs of the Academy of Sciences, 1777.

the requisite differences extend from $\Delta^1 0^0$ only to $\Delta^5 0^0$, and the numerical coefficients of these are of diminished magnitude, and of very easy determination.

The author makes other applications of his theorems; but on these we forbear to enter.

A paper was read by Professor Challis on the Determination of the Longitude of the Cambridge Observatory by Galvanic Signals.

The experiment of which this paper contains the details, was made at the suggestion of the Astronomer Royal, and conducted according to a scheme arranged by him for giving and receiving the signals. A galvanic connexion having been established between the Greenwich Observatory and the Cambridge Telegraph Office, by means of the London central station of the Electric Telegraph Company, signals were sent on the nights of May 17 and 18, 1853, between 11^h and 12^h mean time. The signals were made by causing two needles, one at Greenwich, the other at Cambridge, to start by completing the galvanic circuit at either place of observation. The times of starting were noted at both places, and reduced to the sidereal times of the respective observatories, to serve by comparison for determinations of the difference of their longitudes. On each night the signals were made alternately for a quarter of an hour at one station, in batches containing an arbitrary number of signals not exceeding nine, and then for a quarter of an hour at the other station in a similar manner. On the first night the total number of signals was 151, and on the second night 139. The two observers, Mr. Dunkin of the Greenwich Observatory, and Mr. Todd of the Cambridge Observatory, changed places in the interval between the two nights' observations; Mr. Todd observing at Greenwich, and Mr. Dunkin at Cambridge, on the second night. Also it was arranged that the two observers should observe identical stars on the two nights, as well as the stars ordinarily used for clock errors, and that the same apparent right ascensions of the stars should be employed for reducing the signal-times at both observatories. The Cambridge Observatory time was conveyed with the greatest care to the Telegraph Office at the Cambridge Railway Station by the transfer of three chronometers. By a first calculation, the longitude of the Cambridge Observatory was found to be $23^s \cdot 03$ east of Greenwich.

Professor Challis subsequently made another calculation, taking into account the effect on the times of meridian transits of stars produced by the forms of the transit-pivots, according to a method which he has described in the Memoirs of the Royal Astronomical Society (vol. xix. p. 103). The errors arising from the deviation of the pivots from the cylindrical form being eliminated, the longitude is found to be $22^s \cdot 70$ east of Greenwich, which is less by $0^s \cdot 84$ than the value hitherto adopted.

May 29, 1854.

A paper was read by Professor Fisher, entitled "Additional Observations on the Development of the Vertebral System."

PROCEEDINGS

OF THE

CAMBRIDGE PHILOSOPHICAL SOCIETY.

November 13, 1854.

A paper, by R. L. Ellis, Esq., was read, entitled "Remarks on the Fundamental Principle of the Theory of Probabilities."

Also, "On the Purbeck Strata of Dorsetshire." By the Rev. O. Fisher.

The object of this paper was to describe the beds from which a series of insect remains and other fossils had been collected by the author, and presented to the Woodwardian Museum.

The connexion of the Purbeck beds with the Oolitic rather than with the Wealden series was maintained, while both were shown to be unconformable in this district to the cretaceous system. Reasons were given for thinking that the materials, of which both the Wealden and Purbeck were composed, had travelled from west to east; and the beds of the New Red Sandstone, as they occur in Devonshire, were pointed out as affording a mass of strata which would furnish a detritus of the character of a large portion of the Hastings sands of Hampshire and Dorsetshire.

In describing the Purbeck beds, the author followed the system of the late Professor E. Forbes, dividing them into upper, middle, and lower; and entered into some detail of the alternations of salt and freshwater conditions that prevailed during their deposition. The aspects under which the same beds appear at different points of the district under examination were particularized, and it was attempted to be shown that these were in conformity with the theory of a current setting from the west towards the east. The mode of occurrence of the remains of insects in the middle and lower Purbecks was somewhat minutely described, and it was suggested that some interesting chronological speculations might be grounded upon it.

The paper concluded with an attempt to explain the singular fractured condition of about thirty feet of the lower Purbeck strata throughout the eastern part of the county. It was supposed that this might have been caused by the deposition of sediment upon the remains of the Portland forest before the mass of the trees had been removed by decomposition; the sediment, after it had become consolidated, settling unequally as the carbonaceous matter was gradually removed.

November 27, 1854.

Prof. Willis gave an account of a new form of Atwood's Machine.

December 11, 1854.

A communication was made by Dr. Paget on a case of involuntary tendency to fall forwards.

February 19, 1855.

Mr. Hopkins gave a lecture on certain changes of Terrestrial Temperature, and the causes to which they may be attributed.

March 5, 1855.

Dr. Clark gave an account of some recent discoveries respecting the origin, migrations, and metamorphoses of Entozoa, and their bearing on the notion of spontaneous generation.

April 23, 1855.

A paper was read by the Master of Trinity, on Plato's Survey of the Sciences, contained in the seventh book of the Republic.

Plato, like Francis Bacon, took a review of the sciences of his time; and like him, complained how little attention was given to the philosophy which they involved. The sciences which Plato enumerates are arithmetic and plane geometry, treated as collections of abstract and permanent truths; solid geometry, which he "notes as deficient" in his time, although, in fact, he and his school were in possession of the doctrine of the "five regular solids;" astronomy, in which he demands a science which should be elevated above the mere knowledge of phænomena. The visible appearances of the heavens only suggest the problems with which true astronomy deals; as beautiful geometrical diagrams do not prove, but only suggest geometrical propositions. Finally, Plato notices the subject of harmonics, in which he requires a science which shall deal with truths more exact than the ear can establish, as in astronomy he requires truths more exact than the eye can assure us of. It was remarked also, that such requirements had led to the progress of science in general, and to such inquiries and discoveries as those of Kepler in particular.

May 7, 1855.

The Master of Trinity read a paper on Plato's notion of *Dialectic.* At the end of the survey of the sciences contained in the seventh book of the Republic, which was the subject of a paper at the last meeting, Plato speaks of *Dialectic* as a still higher element of a philosophical education, fitted to lead men to the knowledge of real existences and of the Supreme Good. Here he describes Dialectic by its objects and purpose. In other places Dialectic is spoken of as a method or process of analysis; as in the 'Phædrus,' where Socrates describes a good dialectician as one who can divide a subject according to its natural members, and not miss the joint, like a bad carver. Another Dialogue, in which there are examples given of dividing a subject, is the *Sophistes*, where many examples of dichotomous or bifurcate division are given. But this appears from the Dialogue to have been a practice of the Eleatic rather than of the Platonic school. Aristotle proposed a division of subjects according to his ten *Categories*, which he and others since have extensively used. Xenophon says that Socrates derived *Dialectic* from a term implying to *divide a subject into parts*, which Mr. Grote thinks unsatisfactory as an etymology, but which has indicated a practical connexion in the Socratic school. The result seems to be, that Plato did not establish any method of analysis of a subject as his Dialectic; but he conceived that the analytical habits formed by the comprehensive study of the exact sciences, and sharpened by the practice of dialogue, would lead his students to the knowledge of first principles.

Also, Mr. Maxwell gave an account of some experiments on the mixture of colours.

May 21, 1855.

A paper was read by Mr. Hopkins on the External Temperature of the Earth and the other Planets of the Solar System.

We have not sufficient *data* to determine the superficial temperature of any planet besides our own. We know, however, that it must mainly depend on the temperature of the planetary space, and on the heat which the nearer planets at least receive directly from the sun, but modified, and possibly in a far greater degree than has been generally supposed, by the particular circumstances by which each planet may be characterized. The modifying circumstances more particularly referred to in this paper, are the existence of atmospheres surrounding the planets, the positions of their axes of rotation, and the conductivity and specific heat of the substances forming the outer crust of each planetary body of our system. No astronomer, judging from the appearances which Mars and Jupiter present to us, would entertain any serious doubt as to the existence of atmospheres surrounding those planets, and the probability would seem to be almost equally strong of Saturn being likewise enveloped

in a similar manner. The obliquity of the axis of rotation is known with considerable accuracy in the cases of Mars and Jupiter; and also in that of Saturn, if it coincide with the axis of rotation of his ring. Venus presents great difficulties to the observer, but it appears now to be pretty satisfactorily determined, that the period of rotation about her own axis is nearly the same as that of the Earth, and that the obliquity of her axis is large, amounting to as much as about 75°. This must produce an extraordinary difference between the changes of *annual* temperature in that planet and those which we experience. The author has endeavoured, in this paper, to estimate numerically the effect of this anomalous obliquity. Practical astronomers have entertained the opinion that Venus likewise has an atmosphere. Of Mercury we know too little by direct observation to form any opinion on those points founded on observed facts, and the same remark will apply to the remoter planets beyond Saturn; but most astronomers probably feel much the same conviction that Mercury, Uranus, and Neptune have atmospheres of greater or less extent, as that they revolve round their own axes with greater or less angular velocity.

It is not the author's object, however, to adjust the balance of probabilities for particular hypotheses in favour of planetary atmospheres or against them; but assuming their existence, to estimate their effects on the planetary temperatures. And in like manner he points out the influence which must be exercised by a greater or less conductivity, and specific heat in the superficial matter of a planet, without professing to discuss the probability of such properties being materially different in the different planets. The Earth's atmosphere is known to be almost completely diathermanous for heat radiating directly from the Sun; and it is assumed to be equally so for the heat which proceeds directly from the fixed stars, and to which the general temperature of space is due. This radiating heat therefore has little or no effect in heating the atmosphere during its transmission to the Earth's surface; but after falling upon and heating terrestrial objects, it loses the power of radiating completely through the atmosphere, and is transmitted back into space through the atmosphere by *conduction*, *convection*, and *partial radiation* to limited distances. But for any of these modes of transmission, it is essential that the temperature of the atmosphere should be greater in its lower than in its upper portions, and in a degree greater as the quantity of heat to be transmitted is greater. The temperature (τ_2) of the upper portion must be determined by the condition, that, in a given time, a quantity of heat must radiate from it into surrounding space equal to that which falls upon it from external sources in the same time, and is transmitted back after reaching the surface of the earth or objects near to it. Consequently τ_2 must be independent of the height of the Earth's atmosphere. At lower points the temperature will increase till we reach the surface of the Earth; and if we denote the temperature there by τ_1, it is manifest that τ_1 will be greater, the greater the height of the Earth's atmosphere.

It must here be particularly observed, that τ_2 is the proper tem-

perature of the component particles of the atmosphere, and is probably widely different from the temperature which would be indicated by a thermometer placed at the upper extremity of the atmosphere, since the instrument would not only be affected by the exchange of heat between its bulb and the atmospheric particles, but also by the heat radiating upon its bulb from every source of heat in surrounding space; while the atmosphere, on account of its diathermancy, would remain unaffected by this radiating heat.

Conceive now a thermometer to be placed at a point sufficiently above the earth's atmosphere. If the bulb were sheltered from the direct influence of the solar rays, the thermometer would indicate the temperature of that point of space, independent of the effect of radiation from the central luminary of the solar system, but dependent on the radiation from all other sources of heat in the universe. If the instrument thus sheltered were sufficiently remote from the sun and every planet, it would indicate very nearly the same temperature at every point within the solar system, assuming the absence of all unknown centres of heat within that system or near to it. This is what may be understood by the general temperature of planetary space. Let it be denoted by T. We shall then have T greater than τ_2; and therefore if we now conceive the thermometer to be transported to the upper limit of the atmosphere, it will be affected by the lower temperature there, and will indicate a temperature intermediate to T and τ_2. If the instrument be brought still lower within the atmosphere, it will indicate a still lower temperature, from its being entirely surrounded by a portion of the atmosphere more dense than that at the extreme boundary, till this tendency to lower the indications of the thermometer is counteracted by the greater temperature of the atmospheric particles as we descend towards the Earth's surface. At some point, consequently, within the Earth's atmosphere the indication of the thermometer would attain its *minimum*; after which, in descending continuously towards the Earth, the temperature indicated would constantly increase, omitting variations due to temporary or local causes. Thus it follows that the existence of an atmosphere like that of the Earth, enveloping a planet, may, according to its extent, either elevate the superficial temperature of the planet above, or depress it below that of surrounding space independently of the direct solar radiation. With respect to our own globe, we are entirely ignorant of the height to which the thermometer, in ascending, would continue to indicate a decreasing temperature, but we are sure that such height is great. This is important with reference to the ultimate object of this paper; for if the height of a planet's atmosphere were too small to allow a thermometer descending in it to attain its *minimum* indication, it is manifest that an increase of atmosphere would cause a *decrease* in the planet's superficial temperature; whereas if the height of the atmosphere were great enough to allow the thermometer to attain the *minimum*, any increase of atmosphere would necessarily cause an *increase* in the superficial temperature of the planet. In the Earth's atmosphere, we are sure (as just remarked) that the indications of a thermometer

would constantly increase in its descent from a very high point above the Earth's surface; and therefore it follows, that if a planet be enveloped in an atmosphere similar to that of the Earth, but of greater height, the superficial temperature of that planet will be higher than that of the Earth, supposing both to exist in the planetary space unaffected by the heat which radiates from the Sun; while, on the contrary, the superficial temperature of the planet would necessarily be less, under the same conditions, than that of the Earth, if its atmosphere were smaller, unless it should be so small as not to allow a thermometer descending in it to reach its *minimum* indication. If the planet were entirely without atmosphere, its superficial temperature (in the assumed absence of solar radiation) would be that of surrounding space; but we have no means of determining what relation that temperature bears to existing terrestrial temperature, or to what this latter temperature would become in the absence of solar radiation.

The author has calculated from Poisson's formulæ the increase of temperature in the superficial crust of the Earth, due to the amount of heat received by direct radiation from the Sun, in different latitudes, above that temperature which would be common to all parts of the Earth's surface in the absence of solar radiation, and with a uniformity of intensity of stellar radiation in all directions upon our globe. But this increased temperature must produce an augmentation of temperature in the atmosphere, which must react on the terrestrial temperature till equilibrium of temperature be established. The author has endeavoured to estimate the amount of this indirect effect of solar radiation by means of the data furnished by M. Dove's work on terrestrial temperatures, combined with calculations based on Poisson's formulæ. He concludes that the whole effect of solar heat at any proposed place is very nearly double that due to the immediate and direct effect of solar radiation. Having thus ascertained this entire effect, he finds the temperature which would pervade the whole surface of the earth if the solar heat were extinguished. He estimates this temperature at $-39^{\circ}\cdot 5$ C.

The *annual variation* of temperature in any latitude is found to be nearly the same in amount for the terrestrial surface and for the part of the atmosphere resting upon it. This must be understood as applying to those places at which the temperature is not materially affected by the *horizontal* transference of heat by marine or aërial currents, or any local causes, which disturb the dependence of temperature on latitude alone. The author also points out the dependence of the annual inequalities of the terrestrial temperature (and consequently of those also of the atmosphere) on the conductivity and specific heat of the matter which constitutes the Earth's crust. If these were much greater, the annual changes of temperature would be much less.

Before applying these results to other planets, the author states that he does not admit the notion, that the remoter planets may derive a considerable superficial temperature from the remains of that internal heat which they probably possessed in the earlier stages of

their existence. It is a well-established conclusion, that the superficial temperature of our own globe has arrived at that point below which it can never descend by more than the small fraction of a degree, so long as all *external* conditions remain the same as at present; and the superficial temperature of the remoter planets will in all probability be reduced to the corresponding limit. To these external conditions, therefore, and not to their primitive heat, must the existing temperatures on the surfaces of these planets be attributed, assuming always that they are not of less antiquity than our own globe. Hence the superficial temperature of the Earth, with its present atmosphere, placed at the distance of Neptune, Uranus, or Saturn, would be very nearly $-39^{\circ}\cdot5$ C., since the effect of solar radiation at those distances would be nearly insensible. But if the extent of the atmosphere were increased, the superficial temperature would be augmented in a corresponding degree. Judging by the decrements of temperature observed by Mr. Welsh, the author concludes that an increase in the height of the Earth's atmosphere of 35,000 or 40,000 feet, would elevate her superficial temperature, if placed in the remoter planetary regions, to nearly the mean temperature of our present temperate zone. The same conclusion will hold with respect to the three planets above mentioned, if we suppose them to have atmospheres similar to that of the Earth, and of sufficient extent. Their temperatures must be sensibly uniform over the whole of their surfaces, not being subject to any appreciable *annual* variation.

The same conclusions will apply to Jupiter, except that there will be a small augmentation of temperature arising from solar radiation, which the author calculates might amount to about $2\frac{1}{2}^{\circ}$ C. at his equator.

Hence the author concludes that those views which assign a necessarily low temperature to the above-mentioned planets in consequence of their distance from the Sun, are altogether untenable.

The conditions under which Mars is placed approximate more nearly to those of the Earth than for any other planet. The author calculates, that with an atmosphere similar to that of the Earth, and exceeding it in height by about 15,000 or 20,000 feet, the equatorial temperature of Mars may be about 60° F., or $15^{\circ}\cdot5$ C., and his polar temperature about -10° C. The extent of the *annual* variations would be about half those on our own planet in corresponding latitudes, supposing the conductivity, specific heat, and radiating power of the matter composing his superficial crust to be the same as for the Earth.

Again, if the Earth, with her present atmosphere and *obliquity*, were placed in the orbit of Venus, the mean equatorial temperature would be upwards of 90° C., subject to the reduction, which would doubtless in this case be great, due to the horizontal transference of heat. The mean polar temperature would be about 16° C. A diminution in the atmosphere would reduce these temperatures in any assigned degree. But the *obliquity* of Venus, though not satisfactorily determined, is considered to be much greater than that of

the Earth, amounting, according to the estimate of some astronomers, to as much as 75°, as heretofore stated. This would of course render the character of her seasons entirely different from those of the Earth. The greatest mean annual temperature would be at the pole. Independently of the horizontal transference of heat by aërial currents or other causes, taking the extreme obliquity of 75°, and supposing the atmosphere of Venus to be exactly like that of the Earth, her mean temperature at the equator would be about 56° C., and at the pole 95° C. This latter would probably be much lowered by currents; but if the height of the atmosphere of Venus be less than that of the Earth's atmosphere by about 25,000 feet, the author considers that the mean temperature of Venus in her equatorial regions would not exceed that of the temperate regions of the Earth; while the mean polar temperature would probably be about 40° C., or about 12° or 13° C. higher than the Earth's equatorial temperature. The heat of *sunshine* may be moderated by an atmosphere more laden with vapour than that of the Earth.

Supposing the atmosphere of Venus like that of the Earth in its nature and its magnitude, the temperature at her poles, with the supposed obliquity, must be subject to an enormous *annual* inequality, amounting to between 70° and 80° C. above or below the mean temperature, liable, however, to a great reduction by horizontal transference of heat. It may also be considerably reduced by the nature of the matter which constitutes her outer crust. A reduction, likewise, in the extent of her atmosphere, like that above supposed, would probably diminish the amount of this inequality, as well as the mean temperature, though not in the same degree. It is easy to conceive that the coefficient of the inequality may be thus reduced to some 40° C.; and supposing the mean temperature then, as above estimated, at about 40° C., the *annual* polar temperature will oscillate between 0° C. and 80° C. At the equator, the *semi-annual* inequality might amount, under the above suppositions, to about 10° or 12° C., in which case the equatorial temperature might oscillate between something below zero (C.) and some 25° C. It should be recollected also, that a much greater reduction of the mean temperature would result from a greater reduction in the extent of this planet's atmosphere than above supposed with reference to the height of our own atmosphere. This would not, the author conceives, be inconsistent with the existence of a large quantity of vapour in the atmosphere, affording shelter from the heat and glare of sunshine.

The Moon is under the peculiar circumstances of the absence of a sensible atmosphere, and her long period of rotation about her axis. Assuming her to have no atmosphere at all, the mean temperature of her outer crust, in the absence of the Sun, would be the general temperature of that portion of planetary space in which the solar system is situated. How much this might differ from the superficial temperature which the Earth would have with the like absence of the Sun, and which the author estimates at $-39°\cdot5$ C., as above stated, it is impossible to determine; but whatever it may be, the influence of the Sun's heat would be to increase it by about

40° C. at the Moon's equator, and by a small amount only at her poles. This must be attended by an enormous monthly inequality, amounting to nearly 60° C., supposing the matter of which her superficial crust is composed to have the same conductivity, specific heat, and radiating power as the crust of the Earth. If these be much greater for the Moon, this inequality might be considerably diminished. At the poles it must be comparatively small.

The lunar temperatures here spoken of are those of the matter forming her external crust. The temperature which would be indicated by a thermómeter placed in her immediate vicinity would be affected by the Moon (in the assumed absence of an atmosphere) only by her direct radiation. We have not the means of determining what this temperature may be.

Also a paper was read "On the singular Points of Curves." By Professor De Morgan.

Mr. De Morgan defines a curve as the collection of *all* points whose co-ordinates satisfy a given equation; and contends for this definition as necessary in geometrical *algebra*, whatever limitation may be imposed in algebraic *geometry*. He divides singular points into points of singular *position* and points of singular *curvature*; the character of the former depending on the axes, but not that of the latter. Both species are defined as possessing a notable property, and such as no arc of the curve, however small, can have at all its points.

The form first considered is that of which the case usually taken is an *algebraic* curve. Let $\phi(x, y)$ be a function which for all real and finite values of x and y is real, finite, and *univocal*; let the curve be $\phi(x, y)=0$, considered as an individual of the family $\phi(x, y)=$const. The two curves $d\phi : dx=0$, $d\phi : dy=0$, or $\phi_x=0$, $\phi_y=0$, are the *subordinates* of this system, on which the singular points of all depend.

When ϕ is not reducible to another function of the same kind by extraction of a root, it divides the plane of co-ordinates into *regions* in which, severally, it is always positive or always negative. By this consideration it is easily shown (independently of y', y'', &c.), that if $(x+dx, y+dy)$ be a point on the tangent at (x, y), $\phi(x+dx, y+dy)$ has the sign of $\phi_{xx}dx^2+2\phi_{xy}dxdy+\phi_{yy}dy^2$. Hence, immediately after leaving the curve, ϕ agrees with or differs from $-\phi_y y''$ at the point left, according as the curve is left on the convex or the concave side. Hence easily follow the criteria of flexure, and also the following relation between any two points whatsoever of the curve.

Let two points be called *similar* when a line drawn from one to the other cuts the curve an even number of times (0 included) with the same *abutments* (on convexity or on concavity), or an odd number of times with different abutments. Let other points be called *dissimilar*. These points are similar or dissimilar, according as their values of $\phi_y . y''$ agree or differ in sign.

An *à priori* proof is given that multiple points, cusps, and isolated points, must be determined by $\phi_x=0$, $\phi_y=0$, or can only take place when both subordinates meet the curve. It is shown that, in the

system $\phi(x, y)=$ const., the cusp of $\phi(x, y)=0$ must be an evanescent loop, and the isolated point an evanescent oval, or bounded portion. Some discussion of the meaning of $y'=a+b\sqrt{-1}$ at an isolated point is given.

There have been two methods of treating the singular points. The first has recourse to the theory of equations, using differentiation, if at all, only to supply coefficients. The second attempts canonical forms derived from differential coefficients, and examines, in succession, the meaning and bearing of the successive orders of differential coefficients. Mr. De Morgan affirms that this second method cannot be what it pretends to be; and, by treating it generally, shows that its questions are ultimately dependent upon the theory of equations. An equation of the form $\Sigma A y'^{x}=0$, when it has no equal roots, decides the character of a singular point definitively; and reduces it to a number of intersecting branches without contact, a number of coinciding isolated points without real tangents, or some of one and some of the other. When the equation has some real roots, each set furnishes either multiple branches with contact, or cusps, or conjugate points with real tangents. All this is easily illustrated by examining the curve in which $\phi(x, y)$ is an infinitely small constant, near to the singular point of $\phi(x, y)=0$.

A theorem given by Lagrange, and strongly indicated in the writings of Newton, Taylor, Stirling, Cramer, and John Stewart, but apparently nearly forgotten, solves the question of finding the higher or lower degrees of all the roots of $\Sigma A y^{a}=0$, where A is a function of x of the degree a; that is, where $A=x^{a}(a+A')$, and A' vanishes when $x=\infty$ or when $x=0$. By this theorem (which is also given in the first* Number of the Quarterly Journal of Mathematics), y being $x^{r}(u+U)$, all the values of r, and their corresponding values of u, are very easily found; and repetition of the process upon a transformed equation gives $U=x^{r_{\prime}}(u_{\prime}+U_{\prime})$, and so on. It obviously follows, that when the origin is removed to any singular point of a curve, the discussion of the branches which pass through that point, and of their contacts with the tangent and each other, is made very easy. In proof of this, the author takes the following instance,—

$$x^{12}+x^{14}+x^{11}y-x^{8}y^{2}+2x^{7}y^{3}-x^{4}y^{4}+y^{6}-3xy^{3}+x^{14}y^{13}=0,$$

and discusses its infinite branches, and the sextuple point at the origin (which turns out to be a couple of isolated points, and a cusp of similar flexures), with very much less space and trouble than ordinary methods would demand from a much less complicated instance. It is also shown that the *lower form* of Lagrange's theorem solves the following question:—Given an equation with a certain number of equal roots, what effect will be produced upon these roots by given infinitesimal alterations in the coefficients, how many will remain real, and how many will become imaginary?

Newton has given the foundation and the chief step of a geome-

* There attributed to Mr. Minding, by a mistake caused by M. Serret, who incorporates it with a theorem of Mr. Minding, without any notice of its author.

trical method (*Newton's parallelogram*) which has passed into oblivion, though it occurs in the celebrated second letter to Oldenburg, has been fully described by Stirling, used by Taylor and De Gua, and forms the main method of Cramer's work on curves. Mr. De Morgan proposes to call it the method of *co-ordinated exponents*.

He proceeds to describe and enlarge this method; observing that, of the polygon which represents an equation, Newton and his followers are in full possession of the connexion of the sides with the solutions, and fail only in not grasping the connexion of the whole polygon with the whole equation. Both Newton's method and Lagrange's, the second of which is an arithmetical version of the first, may be applied to irrational equations, but it will be convenient to confine the description to the form $\Sigma ax^m y^n = 0$, where m and n are integers.

In $ax^m y^n$, let n be an abscissa, and m an ordinate, and let (m, n) be called the exponent point of the term $ax^m y^n$. Take some paper ruled in squares (or ruled both ways in any manner, for any equal rectangles will do) to facilitate the process when n and m are always integers, and lay down all the exponent points in $\Sigma ax^m y^n = 0$. Through some of these points draw a *convex* polygon including all the rest, which can only be done in one way. Should the points be so many and so scattered that some *method* must be applied, the geometrical method is a translation of the main arithmetical method of Lagrange's theorem. The points which end on, or otherwise fall in, the sides of the polygon show the *essential* terms of the equation: no others are wanted to determine q and u in $y = x^r(u + \mathrm{U})$. The upper contour of the polygon shows how all the solutions commence in descending powers of x; the under contour does the same for ascending powers. Take any side of either contour, its projection on the axis of n shows the number of roots it represents, the tangent of the angle it makes with the *negative* side of the axis of n shows the value of r.

It will not be needful to abstract the developments given in the paper: we shall only notice the *inverse* method. The following example is taken, and the construction of the equation is even easier (under Cramer's form) than the direct treatment of it. The example chosen by the author is the following:—Required $\phi(x, y) = 0$, of the twelfth dimension in terms of y, such that the twelve roots of y, with reference to lower degrees, shall be as follows: two roots of the degree 1, four of $\frac{1}{2}$, two of 0, one of -1, two of $-\frac{3}{2}$, one of -2. But with reference to higher degrees, there are to be one root of the degree 3, two of $\frac{1}{2}$, three of 0, three of $-\frac{1}{3}$, two of -1, one of -3. On examination these conditions are found compatible, and the most general equation which satisfies the conditions is found.

The paper is terminated by a discussion on the pointed branch, for the admission of which, as a branch altogether composed of singular points, the author contends.

November 12, 1855.

A paper was read by the Master of Trinity on the Intellectual Powers according to Plato.

Also, Prof. Sedgwick gave a lecture on the Classification and Nomenclature of the Palæozoic Rocks.

November 26, 1855.

A paper was read on the Earthquake in Switzerland in July last, by the Rev. O. Fisher.

The 25th of July, 1855, on which the first and most severe shock was felt, was a very wet, close day, and the little wind stirring came from the S.W.

In the Münster Thal the earthquake began by a rumbling vibration like that caused by a carriage run under an archway, gradually increased for about four seconds, and then suddenly ceased. The oscillation seemed to be from E.S.E. to W.N.W., but would be affected by the build of the house.

In the church at Bienne two stones fell from the groining thirty or forty feet into the organ pipes, to a point between 2 and 2½ feet N. by E. of the point vertically beneath their first position; and allowing for the direction of the building, this would give the motion of the earth about from N.E. to S.W. This wave may have been a reflexion caused by the wave entering the Jura from the valley. Another shock was felt at Bienne, at 10 A.M., on the 26th.

The great shock was felt at Strasburg, slightly at Lyons in a direction from E. to W.; likewise at Chambery, Alessandria, and Genoa. The account given by Plana in the *Times* does not seem very intelligible, but as far as can be made out from the stopping of the clocks, it gives the direction of the shock at Turin about 30° W. of S. Chiavenna, the western shore of the Lake of Constance, and Schaffhausen seem to fix the limit to which it was felt towards the east. The area shaken was therefore an oval, having its largest dimension about 300 miles N. and S., and its shortest 250 miles E. and W.

At Geneva the shock appeared to be directed to E.N.E. At Thun it appeared to come from Frutigen. At Kandersteg, at the north foot of the Gemmi, the shock was N. and S. At Interlaken the shocks were more severe; and at Ormont, Canton Vaud, the oscillation came from W. to E., preceded by a noise which lasted for an instant only, and the roof of a house fell in. It seems that nearer the centre of the oval the intensity of the shock was greater. At the baths of Leuk a chimney was thrown down and the walls cracked; but on ascending the valley of the Rhone the evidence of disturbance became rapidly more marked up to Visp, where only seven houses remained habitable. At the little inn, the "Soleil," the flag pave-

ment was burst upwards as if by a blow from beneath: a continual succession of shocks have occurred there at variable intervals up to the present time. Passing on towards Brieg, the evidence of the violence of the shock rapidly diminished. The valley of Zermatt showed the chief disturbance; the bridle road was continually fissured, and in some places slipped down into the valley. At Stalden there was much destruction, but at St. Nicholas the havoc was very great indeed. Higher up the traces of the shock were less and less, until at Tesch, Randa, and Zermatt, there was no mischief done. The other branch of the valley by Saas did not suffer so much.

Drawing lines through the different places in the direetion in which the wave proceeded, it will be found that they converge very nearly to Visp, showing that to be nearly the centre of disturbance.

Mr. Croker of Caius College was walking between Stalden and Visp when the great shock occurred, which appeared to him to be a blow from beneath like the springing of a mine under him, and he observed that the path sunk several inches from the solid rock; a lofty isolated rock on the opposite side of the valley vibrated, and blocks of stone came tumbling down on all sides. The quivering lasted about thirty seconds. He did not observe any sound preceding the shock, though this was heard at Visp; but a crashing sound accompanied the great shock, and a fainter sound continued afterwards beyond the motion. He felt continued shocks from one o'clock till four, when he proceeded towards Sion. At Zermatt the same shock was felt very much less violently, and no sound preceded it; and after attaining its maximum, it ceased somewhat suddenly. It was felt less strongly on the Riffelberg; and on the 27th another, felt at Zermatt as strongly as before, was not felt on the little Mont Cervin.

A sound seems in general to have preceded the earthquake at places near the centre of disturbance: at Visp likened to the echo of an avalanche, but at a distance there was only a sound simultaneous with the shock. The sound may have arisen from the grinding of the walls of the fissure, or whatever violent action may have occurred at the origin, and the sound-waves travelling more rapidly than the earthquake-wave. This is opposed to Mr. Mallet's view, though he gives a table in which the least rate given for sound travelling through any kind of stone is 3640 feet per second, while the rate of motion of the earthquake of Lisbon was 1750 feet. If the view stated be correct, the disturbance must have been deep in the earth, which would also explain the upward blow felt by Mr. Croker. At greater distances the sound-wave would be expended sooner than the earthquake-wave, and the accompanying sound be due to local action.

Chimneys and such like structures appear to have fallen away from the centre of disturbance, being thrown down by the return stroke of the wave; the forward stroke having to move them only from a state of rest, whereas the return stroke would have to overcome the momentum generated by the former.

Near the centre the shock was sudden, passing away gradually.

At a distance it began with slight quivering, gradually attained a maximum, and then suddenly ceased. Now if the disturbance occurred along a large fissure, perhaps several miles in length, and of unknown depth, the waves from different portions would reach any given point in succession, and at intervals the combined effect of many waves would be felt, producing a result analogous to the rolling of thunder due to the varying distance of the source of sound, while the sudden concussion at a nearer point is like the detonation heard when the lightning is near the auditor.

The shocks were less severe in the mountains than in the valleys. As far as the wave progressing horizontally is concerned, it would, on entering a mountain, at first be nearly bounded by a horizontal plane continuous with that of the valley, just as light is propagated in straight lines; but there would also be a diversion of a part (analogous to the diffraction of light at a screen) into the mountain, so that where the wave passed for some distance into a range it would finally be felt at the summit. It is observable that the shock on the 25th was less severe on the Riffelberg than at Zermatt, yet it travelled through the mountain and was felt at Turin.

The period of elevation of the Alps seems about contemporaneous with the older Pliocene of Sir C. Lyell. The country is broken up with faults, which probably there, as elsewhere, follow the lines of valleys. The valley of Visp lies in the axis of two ranges which have all the appearance of a mighty valley of elevation. The shock may have arisen from a shifting of the beds on this line of ancient disturbance, and very probably the somewhat rectangular corner between the valleys at Visp suffered the principal displacement. Earthquakes in non-volcanic regions probably arise from a failure of support. At the period of the elevation of the Alps, the more heated lower parts of the earth's crust must have come nearer to the surface than their normal position, and contractions and failure of support must occur while cooling, and the comparatively recent elevation of the Alps may give reason for thinking this to be still going on.

December 10, 1855.

A paper was read by Mr. Maxwell on Faraday's Lines of Force.

The method pursued in this paper is a modification of that mode of viewing electrical phænomena in relation to the theory of the uniform conduction of heat, which was first pointed out by Professor W. Thomson in the Cambridge and Dublin Mathematical Journal, vol. iii. Instead of using the analogy of heat, a fluid, the properties of which are entirely at our disposal, is assumed as the vehicle of mathematical reasoning. A method is given by which two series of surfaces may be drawn in the fluid so as to define its motion completely. The uniform motion of an imponderable and incompressible fluid permeating a medium, whose resistance is directly as the velocity, is then discussed, and it is shown how a system of surfaces of equal pressure

may be drawn, which, with the two former systems of surfaces, divides the medium into cells, in each of which the same amount of work is done in overcoming resistance. It is then shown that if the fluid be supposed to emanate from certain centres, and to be absorbed at others, the position of these centres can be found when the pressure at any point is known; and that when the centres are known, the distribution of pressures may be found. Methods are then given by which the motion of the fluid out of one medium into another, the resistance of which is different, may be conceived and calculated, and the theory of motion in a medium in which the resistance is different in different directions is stated.

The mathematical ideas obtained from the fluid are then applied to various parts of electrical science. It is shown that the expression for the electrical potential at any point is identical with that of the pressure in the fluid, provided that "sources" of fluid are put instead of positive electrical "matter," and centres of absorption or "sinks" for negative "matter."

The theory of Faraday with respect to the effect of dielectrics in modifying electric induction, is illustrated by the case of different media having different conducting power; and it is shown, that, in order to calculate the effects by the ordinary formulæ of attractions, we must alter in a certain proportion the quantities of electricity within the dielectric, and conceive an imaginary distribution of electricity over the surface which separates it from the surrounding medium.

The theory of magnets and of the phænomena of paramagnetic and diamagnetic bodies is expressed with reference to the "lines of inductive magnetic action;" and elementary proofs of the tendency of paramagnetic bodies toward places of stronger magnetic action, and of diamagnetic bodies toward places of weaker action, are given. This distinction of paramagnetic and diamagnetic is not here used absolutely, but indicates a greater or less conductivity for the lines of inductive action than that of the surrounding medium.

The magnetic phænomena of crystals are then examined, and referred to unequal magnetic conductivity in different directions; and the case of a crystalline sphere in a uniform field of force is worked out.

The laws of electric conduction, as laid down by Ohm, are shown to agree with those of the imaginary fluid, and definitions of quantity and intensity are given, which will apply to magnetism as well as galvanism.

The theory of the attractions of closed circuits, as established by Ampère, is shown to lead to the following results:—

1. The total intensity of the magnetizing force estimated along any closed curve embracing the circuit is a measure of the quantity of the current.

2. The quantity of the current, multiplied by the quantity of inductive magnetic action, from whatever source, which passes through it, gives what may be called the potential of the circuit. The tendency of the resultant forces is to increase this potential.

The theory of Faraday with respect to the induction of currents in closed circuits takes the following form:—

When the quantity of inductive magnetic action which passes through a given circuit changes in any way, an electromotive force proportional to the rate of change acts in the circuit, and a current is produced whose quantity is the electromotive force divided by the total resistance of the circuit.

The mathematical discussion of the electro-magnetic laws is reserved for another communication.

PROCEEDINGS

OF THE

CAMBRIDGE PHILOSOPHICAL SOCIETY.

February 11, 1856.

A paper was read by Mr. Maxwell of Trinity College, "On Faraday's Lines of Force," in continuation of a former paper (Proceedings of the Society, Dec. 10, 1855).

This paper was chiefly occupied with the extension of the method of lines of force to the phænomena of electro-magnetism, by means of a mathematical method founded on Faraday's idea of an "electrotonic state."

In order to obtain a clear view of the phænomena to be explained, we must begin with some general definitions of *quantity* and *intensity* as applicable to electric currents and to magnetic induction. It was shown in the first part of this paper, that electrical and magnetic phænomena present a mathematical analogy to the case of a fluid whose steady motion is affected by certain moving forces and resistances. [The purely imaginary nature of this fluid has been already insisted on.] Now the amount of fluid passing through any area in unit of time measures the *quantity* of action over this area; and the moving forces which act on any element in order to overcome the resistance, represent the total *intensity* of action within the element.

In electric currents, the *quantity* of the current in any given direction is measured by the quantity of electricity which crosses a unit area perpendicularly to this direction; and the intensity, by the resolved part of the whole electromotive forces acting in that direction. In a closed circuit, whose length $=l$, coefficient of resistance $=k$, and section $=C^2$, if F be the whole electromotive force round the circuit, and I the whole quantity of the current,

$$\frac{I}{C^2}lk = F, \quad I = \frac{C^2F}{lk}.$$

The laws of Ohm with respect to electric currents were then applied to cases in which the conducting power of the medium is different in different directions. The general equations were given and several cases solved.

In magnetic phænomena, the distinction of quantity and intensity is less obvious, though equally necessary. It is found, that what Faraday calls the quantity of inductive magnetic action over any

surface, depends only on the *number* of lines of magnetic force which pass through it, and that the total electromotive effect on a conducting wire will always be the same, provided it moves across the same system of lines, in whatever manner it does so. But though the quantity of magnetic action over a given area depends only on the number of lines which cross it, the intensity depends on the force required to keep up the magnetism at that part of the medium; and this will be measured by the product of the quantity of magnetization, multiplied by the coefficient of resistance to magnetic induction in that direction.

The equations which connect magnetic quantity and intensity are similar in form to those which were given for electric currents, and from them the laws of diamagnetic and magnecrystallic induction may be deduced and reduced to calculation.

We have next to consider the mutual action of magnets and electric currents. It follows from the laws of Ampère, that when a magnetic pole is in presence of a closed electric circuit, their mutual action will be the same as if a magnetized shell of given intensity had been in the place of the circuit and been bounded by it. From this it may be proved, that (1) the *potential* of a magnetized body on an electric circuit is measured by the *number* of lines of magnetic force due to the magnet which pass through the circuit. (2) That the total amount of work done on a unit magnetic pole during its passage round a closed curve embracing the circuit depends only on the *quantity* of the current, and not on the form of the path of the pole, or the nature or form of the conducting wire.

The first of these laws enables us to find the forces acting on an electric circuit in the magnetic field. Give the circuit any displacement, either of translation, rotation, or disfigurement, then the difference of potential before and after displacement will represent the force urging the conductor in the direction of displacement. The force acting on any element of a conductor will be perpendicular to the plane of the current and the lines of magnetic force, and will be measured by the product of the quantities of electric and magnetic action into the sine of the angle between the direction of the electric and magnetic lines of force.

The second law enables us to determine the quantity and direction of the electric currents in any given magnetic field; for, in order to discover the quantity of electricity flowing through any closed curve, we have only to estimate the work done on a magnetic pole in passing round it. This leads to the following relations between $\alpha_1\,\beta_1\,\gamma_1$, the components of magnetic intensity, and $a_2\,b_2\,c_2$, the resolved parts of the electric current at any point,

$$a_2 = \frac{d\beta_1}{dz} - \frac{d\gamma_1}{dy}, \quad b_2 = \frac{d\gamma_1}{dx} - \frac{d\alpha_1}{dz}, \quad c_2 = \frac{d\alpha_1}{dy} - \frac{d\beta_1}{dx}.$$

In this way the electric currents, if any exist, may be found when we know the magnetic state of the field. When $\alpha_1 dx + \beta_1 dy + \gamma_1 dz$ is a perfect differential, there will be no electric currents.

Since it is the *intensity* of the magnetic action which is immedi-

ately connected with the *quantity* of electric currents, it follows that the presence of paramagnetic bodies, like iron, will, by diminishing the total resistance to magnetic induction while the total intensity is constant, increase its quantity. Hence the increase of external effect due to the introduction of a core of soft iron into an electric helix.

From the researches of Faraday into the induction of electric currents by changes in the magnetic field, it appears that a conductor, in cutting the lines of magnetic force, experiences an electromotive force, tending to produce a current perpendicular to the lines of motion and of magnetic force, and depending on the number of lines cut by the conductor in its motion.

It follows that the total electromotive force in a closed circuit is measured by the *rate of change* of the number of lines of magnetic force which pass through it; and it is indifferent whether this change arises from a motion of this circuit, or from any change in the magnetic field itself, due to changes of intensity or position of magnets or electric currents.

This law, though it is sufficiently simple and general to render intelligible all the phænomena of induction in closed circuits, contains the somewhat artificial conception of the number of lines *passing through* the circuit, exerting a physical influence on it. It would be better if we could avoid, in the enunciation of the law, making the electromotive force in a conductor depend upon lines of force external to the conductor. Now the expressions which we obtained for the connexion between magnetism and electric currents supply us with the means of making the law of induced currents depend on the state of the conductor itself.

We have seen that from certain expressions for magnetic intensity we could deduce those for the quantity of currents, so that the currents which pass through a given closed curve may be measured by the total magnetic intensity round that curve. Here we have an integration *round the curve itself* instead of one *over the enclosed surface*. In the same way, if we assume the mathematical existence of a state, bearing the same relation to magnetic quantity that magnetic intensity bears to electric quantity, we shall have an expression for the quantity of magnetic induction passing through a closed circuit in terms of quantities depending on the circuit itself, and not on the enclosed space.

Let us therefore assume three functions of $x\,y\,z$, $\alpha_0\,\beta_0\,\gamma_0$, such that $a_1\,b_1\,c_1$ being the resolved parts of magnetic quantity,

$$a_1=\frac{d\beta_0}{dz}-\frac{d\gamma_0}{dy},\quad b_1=\frac{d\gamma_0}{dx}-\frac{d\alpha_0}{dz},\quad c_1=\frac{d\alpha_0}{dy}-\frac{d\beta_0}{dx};$$

then it will appear that if we assume $\frac{d\alpha_0}{dt}$, $\frac{d\beta_0}{dt}$, $\frac{d\gamma_0}{dt}$ as the expressions for the electromotive forces at any point in the conductor, the total electromotive force in any circuit will be the same as that expressed by Faraday's law. Now as we know nothing of these inductive effects except in closed circuits, these expressions, which are

true for closed currents, cannot be inconsistent with known phænomena, and may possibly be the symbolic representative of a real law of nature. Such a law was suspected by Faraday from the first, although, for want of direct experimental evidence, he abandoned his first conjecture of the existence of a new state or condition of matter. As, however, we have now shown that this state, as described by him (Exp. Res. (60.)), has at least a mathematical significance, we shall use it in mathematical investigations, and we shall call the three functions α_0, β_0, γ_0, the *electrotonic functions* (see Faraday's Exp. Res. 60. 231. 242. 1114. 1661. 1729. 3172. 3269.).

That these functions are otherwise important may be shown from the fact, that we can express the potential of any closed current by the integral

$$\int\left(a_2\,\alpha_0\frac{dx}{ds}+b_2\beta_0\frac{dy}{ds}+c_2\gamma_0\frac{dz}{ds}\right)ds,$$

and generally that of any system of currents in a conducting mass by the integral

$$\int\int\int(\alpha_0 a_2+\beta_0 b_2+\gamma_0 c_2)dx\,dy\,dz.$$

The method of employing these functions is exemplified in the case of a hollow conducting sphere revolving in a uniform magnetic field (see Faraday's Exp. Res. (160.)), and in that of a closed wire in the neighbourhood of another in which a variable current is kept up, and several general theorems relating to these functions are proved.

February 25, 1856.

A paper was read, "On a direct method of estimating Velocities, Accelerations, and all similar magnitudes with respect to Axes moveable in any manner in Space, with applications." By Mr. Hayward, of St. John's College.

The frequent recurrence, in many different investigations of kinematics and dynamics, of exactly corresponding equations, suggests the inquiry whether they do not result from some common principle, from which they may be deduced once for all. An investigation based on this idea forms the first part of this paper, and the result is the method mentioned in the title.

This calculus shows how the variations of any magnitude, capable of representation by a straight line of definite length in a definite direction, and subject to the *parallelogrammic* law of combination, may be *simply* and *directly* determined relatively to any axes whatever. If such a magnitude (u) be estimated in a given direction, its intensity in that direction will be represented by the projection on it of the line which represents u. If this given direction be not fixed, but move according to a given law, the projection of u upon it will change by the alteration of its inclination to the direction of u; and the rate of that change is easily calculated, whence an expres-

sion for the acceleration of the resolved part of u along a given axis as *due to the motion of that axis.* If u itself be variable, its variations may be conceived to be due to an acceleration f in a definite direction, which in the time dt produces a quantity fdt in the direction of f to be combined with u by the parallelogrammic law; hence result expressions for the changes in intensity and direction of u. If, u being variable, the variations in its intensity estimated along a given moveable direction be sought, it will consist of two parts: one, that due to the resolved part of f in the given direction; the other, that due to the motion of the axis, which is the same as if f had not existed, or u had been constant: hence expressions for the total acceleration of the resolved part of u along the given moveable axis. If u be resolved along three rectangular axes, these expressions take the forms of familiar kinematical and dynamical equations.

These results furnish immediately expressions for the relative velocities of a point with respect to moving axes when its absolute velocities in their directions are given, and *vice versâ*. They also furnish very ready means of estimating accelerations in variable directions; as, for instance, the radial and transversal accelerations of a point moving in a plane or in space, or the tangential and normal accelerations in the same case. These are some kinematical applications of the calculus.

The dynamical applications form the second part of the paper. Here the general problem of the motion of a system, so far as it is due to *external* forces, is divided into two steps: one from *force* to *momentum*, the other from *momentum* to *velocity*. If the momenta of the particles of a system be reduced like a system of forces, they produce a single *linear momentum* and a single *angular momentum*, just as a system of forces produces a single force and a single couple. The linear momentum is (in our received language) the momentum of the mass of the system collected at its centre of gravity; the angular momentum is a magnitude the constancy of whose intensity in a given axis is equivalent to the assertion of the principle of the conservation of areas for that axis, and the constancy of whose directions determines the "invariable plane" as a plane perpendicular to it. The momentum, whether linear or angular, is a magnitude to which the previous calculus applies, and the resultant force and resultant couple are respectively the accelerators of the two kinds of momentum: hence the equations obtained in the first part, interpreted with respect to these magnitudes, furnish equations in any required form for the determination of the momenta at any instant. The step from force to momentum is independent of the nature of the system, that from momentum to velocity requires the system to be particularized. In the paper the case of an invariable system only is considered, and in particular its motion of rotation about its centre of gravity. The axis of rotation or angular velocity is related in direction to that of angular momentum, as the radius of the central ellipsoid with which it coincides to the normal at its extremity. Hence an angular momentum constant in intensity and direction, in general gives rise to an angular velocity variable in both respects,

and *vice versâ*. The question then becomes, to determine the acceleration of angular velocity due to the motion of the system. This is obtained by determining the acceleration of angular momentum for a line fixed in the body, which is then shown to be a maximum for the normal to the plane containing the axes of angular momentum and velocity; then the acceleration along this line is the total acceleration of angular momentum due to the motion, and the acceleration of angular velocity determined from it (just as the angular velocity is determined from the momentum) is that due to the motion of the system. Also the acceleration of angular velocity due to the forces is related to the resultant couple and its axis, just as the angular velocity to the angular momentum. Thus the accelerations of angular velocity due both to the motion and to the forces being determined, the intensity and direction of the angular velocity at any time is to be found by combining these effects by integration. The problem is worked out in the case of the axis of the resultant couple being coincident with that of angular momentum, so that this remains fixed. The paper concludes with a simple solution of the problems of Foucault's gyroscope as applied to show the effects of the earth's rotation, the simplicity arising from the method of this paper enabling us at once to refer the motion to those axes (neither fixed in the body nor in space) whose motion it is desired to determine.

March 10, 1856.

A paper was read by L. Barrett, Esq., "On the Distribution of the Mollusca on the Coast of Norway." (*Vide* Annals of Nat. Hist. May 1856.)

In this the author observed, that when the fauna of the coast of Norway is compared with that of the other side of the North Atlantic, a great difference will at once be perceived, not only in the number of species, but also in the different distribution of northern and southern types; the Mollusca of Greenland being peculiarly arctic, those of Scandinavia a mixture of southern and northern species. In the southern part of Norway we find the species living on our coasts abundant; but they become rarer as we go north, their place being supplied by arctic forms. Many of the northern species have a great geographical range, at which we need not be surprised when we consider their great antiquity, many of them having existed since the pliocene period; and, in the author's opinion, whenever we find a species with a great geographical range, we may at once infer that it has continued to live from a remote period. It is extremely difficult, according to the present state of the currents in the northern seas, to account for the wide distribution of arctic shells on this side of the North Atlantic; but when we consider that at not a very distant period the temperature and other conditions of this area were totally different, that a cold climate prevailed, certainly accompanied by a current setting from the north (as is fully proved by the

fact that boulders are always found nearly south of the mountain ranges from which they have been originally transported), and that many of the shells are found fossil in the Sicilian tertiaries, this wide distribution may be fully accounted for. As these frigid conditions were gradually altering to more genial ones, those species requiring a lower temperature would gradually die out, and only continue to exist in higher latitudes. The littoral and shallow water species would be most affected by such an alteration of climate; and while the fauna of the littoral and laminarian zones would be entirely changed,—the shells composing that fauna replaced by other species,—those living in the deep sea would continue to exist, perhaps at a greater depth, mingled with the species brought in with the new physical conditions of the area. This we know to be the case; for while the northern littoral shells, such as *Mya truncata*, &c., are found only fossil in Sicily, many of the deep-sea arctic species that existed there when those fossils were alive are still found living in the deeper parts of the Mediterranean.

The same thing occurs on our coasts, where the arctic littoral or shallow-water shells, as *Astarte arctica*, *Tellina proxima*, *Natica helicoides*, &c., which are found in shallow water on the Scandinavian or Greenland coasts, are now rare as deep-sea shells, and that in the same area in which they were formerly abundant as shallow-water species. Some species are capable of enduring great differences of climate, the *Mytilus edulis* being found as abundantly on the coast of Greenland as on our own shores.

It is not difficult to account for the presence of the southern species on the coast of Norway, as the Gulf-stream sets directly along the coast, warming its waters, and rendering them habitable for species requiring a moderately high temperature. The great abundance and wide distribution of these species show that the present order of things has continued for a great length of time. The gradual extinction of northern shells on our coasts is still going on; the number of living specimens of *Pecten danicus* is very small, while dead shells are very abundant, and fresh dead specimens of *Pecten islandicus* are frequently dredged, though a living specimen has not yet occurred. It is probable that this species has died out very recently.

On the eastern shores of Davis's Straits the Mollusca are about half as numerous as on the coast of Norway. The fauna differs in the prevalence of arctic types and the total absence of southern. At a former period the fauna was of a mixed character; species now inhabiting more southern latitudes are found fossil in the raised beaches at Disco Island, which species are no longer found living on the coasts.

April 28, 1856.

A paper was read "On the Theory of Heat," by Mr. A. A. Harrison, of Trinity College.

The object of this paper was to show that there is considerable

reason for supposing that radiant heat is identical with light, and that they both consist of vibrations of the ultimate particles of matter.

There is a strong presumption of this from the facts, that every body heated to a certain temperature, dependent only on the nature of the surface, emits light as well as heat; and that "whenever light manifests itself, heat appears along with it" (Kelland): the difference between radiant heat and ordinary heat is, that radiant heat is due to vibrations in planes normal to its direction of propagation, and that ordinary heat consists of vibrations in all three dimensions.

The author endeavoured to show, in the first place, that the motions of the particles of matter, which must be caused by friction, or in the union of two gases in combustion, is sufficient of itself to account for the following phænomena of heat:—

I. That a body once heated continues of the same temperature, with the exception of heat lost by radiation, conduction, &c. This follows immediately from the principle, that in any system of particles held together by mutual attractions and repulsions, the *vis viva* is independent of the time, and depends merely on the position of the particles.

II. That bodies expand by heat.

Before proceeding to this, the author argued that in gases the repulsive force varies inversely as the cube, and not, as usually stated, the simple power of the distance; that it is not true, without some limitation, that the force varies as the inverse first power, was urged from the fact that such a force would decrease more slowly than one varying as the inverse square, and consequently would be the force observed in astronomical phænomena; and even the oxygen of the ocean would repel that of the air instead of attracting it. That the force varies as the inverse cube was deduced from the law of elasticity, that the density varies as the pressure; for if a particle repels other particles with a force varying as the inverse cube, it repels a fixed plane of them with a force varying as the inverse first power. That this is the case may be seen, by considering that though the particle repels particles similarly situated with a force varying as the inverse cube, yet the number of such particles varies directly as the square of the distance, and therefore the whole effect upon the plane varies inversely as the first power. And if this is true for a plane, it is also true for the solid side of the containing vessel; for any solid may be considered as made up of a succession of planes.

The law being the inverse cube, it follows that in any position the sum of the forces exerted by any particle on two particles, one on each side of it, is least when that particle is half-way between them, and increases the further the particle is removed from the middle point. This is seen directly, for the value of $\frac{\mu}{(a+x)^3}+\frac{\mu}{(a-x)^3}$ is least when $x=0$, and increases until $x=a$. And therefore, in order to produce the same force, it would be necessary that the mean distance should be increased; and hence if the particles of any aëriform body be in motion, the force exerted by them would be greater than when at rest; that is, if the pressure to be supported be con-

stant, the average distance of the particles must increase, and the body must expand.

III. That every aëriform body not in contact with a liquid expands in the same proportion. This was accounted for by the circumstance, that the increase of pressure depends only upon the ratio of the disturbance to the original distance, and not at all upon the absolute distance.

IV. That air and elastic fluids give out heat on compression. By compression the absolute distance of the particles from one another is diminished; but the absolute motion remaining the same, the relative motion is increased.

V. That the same amount of heat is generated in two gases subjected to the same pressure; for the absolute distance of the particle in both being diminished in the same proportion, and the absolute motion remaining unaltered, the relative motion is increased in the same proportion in both.

VI. The specific heats are inversely as the atomic weights. Here it was necessary to show that mass is not necessarily proportional to the quantity of matter, as usually stated; or rather, that a body may have a different mass when considered with regard to the molecular force from what it has with respect to the force of gravity. With regard to elasticity of gases, the weight of any single particle is so small as not to affect the result. The question remains, whether we know anything of the masses of different particles relatively to this repulsive force. To determine their masses we have these data. In several different gases equivalent volumes under the same pressure occupy the same space, that is (assuming the Daltonian theory, that equivalent volumes contain the same number of particles), that each particle of the two different gases exerts an equal pressure on the adjacent particles: and hence, with reference to this law, the mass of a single particle in each of these two different gases is the same, and therefore the "*vis viva*" of equivalent weight or volumes subject to the same motion is the same for both; that is, the quantity of heat of an equivalent of each is the same, and therefore the specific heat of a given weight is inversely as its equivalent number or atomic weight.

With reference to the phænomena of radiation, it may be shown from theoretical considerations that the inverse cube is the law required. The inverse first is impossible, for then there could be no vibrations. For the same reason the inverse second is impossible (Camb. Phil. Trans. vol. vii. p. 98). The inverse fourth is also impossible, for then there could be no vibrations, and the velocity would be infinite (vol. vi. p. 325). It has also been shown that neither the second nor the fourth would satisfy the conditions of the equations (vol. vii. p. 419). Hence, from the theory of radiation, it is supposed that the luminiferous æther consists of solid particles, attracting one another with a force varying as the inverse square (vol. vii. p. 110), and repelling with a force varying as the inverse cube.

Now from the Daltonian theory, and the law of elastic fluids, it

has been shown that the ultimate particles of our atmosphere compose such an æther. But if our atmosphere is the luminiferous æther, we must next inquire whether it does pervade space. Omitting variations of temperature, and merely considering the atmosphere as subject to the two forces of elasticity and gravity, we have for the equation of a column of air on a unit of surface,

$$dp = -\frac{ga^2}{z^2}\rho dz, \text{ or } \frac{1}{p}\frac{dp}{dz} = \frac{kga^2}{z^2}, \text{ where } k = \frac{\rho}{p}.$$

Integrating this, we find that p and ρ, though they become extremely small, never vanish; and therefore, if these laws are absolutely true, our atmosphere does pervade space.

It may be well to obviate the objection, that black substances radiate heat best, and white substances light. This arises from employing the same word radiation to denote two different things: by radiated heat is meant heat given out from a heated body; by radiated light is meant the secondary radiation from the surface of a body exposed to light.

If Sir J. Leslie's experimental calculation of the heat lost from the sun be correct, there is no need of any theories to account for its generation.

From the foregoing arguments and facts, it was urged that motions and forces, which certainly exist in cases of combustion, would produce phænomena exactly similar to those of heat, and therefore that part of the phænomena usually attributed to heat are due to this motion; and if part of them, probably the whole. And further, that if the phænomena of radiation of heat are explained by this motion of the particles of matter, light is simply radiated heat of considerable intensity; and that imponderable substances, whether under the names of æther, caloric, or phlogiston, are equally imaginary.

Also, a paper was read "On the Question—What is the Solution of a Differential Equation?" By Professor De Morgan.

This paper is a short supplement to § 3 of a paper on some points of the integral calculus (Camb. Trans. vol. ix. part 2). It discusses the principles on which such an equation as $y'^2 = a^2$, giving

$$(y - ax + b)(y + ax + c) = 0,$$

is generally affirmed to be completely solved when $b = c$. It dwells on the distinction between a *relation* and an *equation*, which may express the alternative of one or more relations; it points out several cases in which conclusions applicable to the simple relation only are affirmed of any equation; and, with reference to the question asked in the title, discusses the manner in which the answer depends on the cross-question, what degree of discontinuity is allowed to be implied in the word *solution*?

May 12, 1856.

A paper was read by Mr. Warburton "On Self-repeating Series," in continuation of a former paper.

The author showed in his former paper on self-repeating series, printed in vol. ix. part 4. of the 'Transactions' of the Society, that in the fraction which generates a series of either of the following forms,

$$1^{2n} \pm 2^{2n} . t + 3^{2n} . t^2 \pm \text{\&c.} \ldots,$$

or

$$1^{2n+1} \pm 2^{2n+1} . t + 3^{2n+1} t^2 \pm \text{\&c.} \ldots,$$

the numerator of such fraction is a recurrent function of t. He also then determined the coefficients of the several powers of t in such numerator to be given linear functions of the differences (as the case may be) of 0^{2n}, or of 0^{2n+1}.

In his present paper, from the n pairs of equal coefficients which the recurrent numerator contains, the author obtains n linear equations between the $2n$ differences concerned; and selecting any n of these differences, he concludes that each of them can be expressed in terms of the other n differences not so selected; and consequently that no formula, expressed in terms of the differences of 0^{2n} or 0^{2n+1}, need contain more than n of those differences.

He gives the equations requisite for obtaining $\Delta^{n+p}(0^{2n})$ in terms of $(\Delta^n, \Delta^{n-1}, \ldots \Delta^2, \Delta^1)0^2$; and $\Delta^{n+1+p}(0^{2n+1})$, in terms of $(\Delta^{n+1}, \Delta^n, \ldots \Delta^3, \Delta^2)0^{2n+1}$; and he applies these and other of his equations to the elimination of particular differences of zero from sundry formulas.

Also, Mr. Bashforth exhibited models illustrating the Moon's motion.

Also, a paper was read by Mr. Maxwell "On the Elementary Theory of Optical Instruments."

The object of this communication was to show how the magnitude and position of the image of any object seen through an optical instrument could be ascertained without knowing the construction of the instrument, by means of data derived from two experiments on the instrument. Optical questions are generally treated of with respect to the pencils of rays which pass through the instrument. A pencil is a collection of rays which have passed through one point, and may again do so, by some optical contrivance. Now if we suppose all the points of a plane luminous, each will give out a pencil of rays, and that collection of pencils which passes through the instrument may be treated as a *beam* of light. In a pencil only one ray passes through any point of space, unless that point be the focus. in a beam, an infinite number of rays, corresponding each to some point in the luminous plane, passes through any point; and we may,

if we choose, treat this collection of rays as a pencil proceeding from that point. Hence the same beam of light may be decomposed into pencils in an infinite variety of ways; and yet, since we regard it as the same collection of rays, we may study its properties as a beam independently of the particular way in which we conceive it analysed into pencils.

Now in any instrument the incident and emergent beams are composed of the same light, and therefore every ray in the incident beam has a corresponding ray in the emergent beam. We do not know their path within the instrument, but before incidence and after emergence they are straight lines, and therefore any two points serve to determine the direction of each.

Let us suppose the instrument such that it forms an accurate image of a plane object in a given position. Then every ray which passes through a given point of the object before incidence passes through the corresponding point of the image after emergence, and this determines one point of the emergent ray. If at any other distance from the instrument a plane object has an accurate image, then there will be two other corresponding points given in the incident and emergent rays. Hence if we know the points in which an incident ray meets the planes of the two objects, we may find the incident ray by joining the points of the two images corresponding to them.

It was then shown, that if the image of a plane object be distinct, flat, and similar to the object for two different distances of the object, the image of any other plane object perpendicular to the axis will be distinct, flat, and similar to the object.

When the object is at an infinite distance, the plane of its image is the *principal focal plane*, and the point where it cuts the axis is the *principal focus*. The line joining any point in the object to the corresponding point of the image cuts the axis at a fixed point called the *focal centre*. The distance of the principal focus from the focal centre is called the *principal focal length*, or simply the *focal length*.

There are two principal foci, &c. formed by incident parallel rays passing in opposite directions through the instrument. If we suppose light always to pass in the same direction through the instrument, then the focus of incident rays when the emergent rays are parallel is the *first* principal focus, and the focus of emergent rays when the incident rays are parallel is the *second* principal focus. Corresponding to these we have first and second focal centres and focal lengths.

Now let Q_1 be the focus of incident rays, P_1 the foot of the perpendicular from Q_1 on the axis, Q_2 the focus of emergent rays, P_2 the foot of the corresponding perpendicular, F_1F_2 the first and second principal foci, A_1A_2 the first and second focal centres, then

$$\frac{P_1F_1}{A_1F_1}=\frac{P_1Q_1}{P_2Q_2}=\frac{F_2P_2}{F_2A_2},$$

lines being positive when measured in the direction of the light.

Therefore the position and magnitude of the image of any object is found by a simple proportion.

In one important class of instruments there are no principal foci or focal centres. A telescope in which parallel rays emerge parallel is an instance. In such instruments, if m be the angular magnifying power, the linear dimensions of the image are $\frac{1}{m}$ of the object, and the distance of the image of the object from the image of the object-glass is $\frac{1}{m^2}$ of the distance of the object from the object-glass. Rules were then laid down for the composition of instruments, and suggestions for the adaptation of this method to second approximations, and the method itself was considered with reference to the labours of Cotes, Smith, Euler, Lagrange, and Gauss on the same subject.

November 6, 1856.

A paper was read by Dr. Donaldson "On the Structure of the Athenian Trireme, considered with reference to certain difficulties of Interpretation."

The author's intention was to show in this paper that the arrangements for seating the three tiers of rowers in the trireme, which Dr. Arnold has called "an indiscoverable problem," may be adequately explained by an examination of the terms which are used to discriminate the rowers, and of other words referring to the different parts of the war-galley. The name of the *zygitæ*, or rowers of the middle tier, implies that they sat on the ζυγά, or transverse planks connecting the opposite sides of the vessel, also called σέλματα, and in earlier times κληῖδες. The *thalamitæ*, or rowers of the lowest tier, must, in accordance with their name, have had their seats attached to the ribs of the vessel in the θάλαμος, or hold. And the *thranitæ*, or rowers of the highest tier, sat on θρῆνυες, or benches like low stools, extending for seven feet along the alternate ζυγά. The *epibatæ*, or marines, whether as working the supernumerary oars, or as fighting, occupied platforms running along the bulwarks. This view of the matter explained the fact that there was a gangway from the stern to the prow for the passage of the officers, &c. along the σέλματα or ζυγά, between the ends of the stools on which the thranitæ sat. This gangway was called the σελίς, and the same name was given to the passages leading down to the orchestra from the upper part of the theatre between the rows of seats occupied by the spectators. Hence was derived a philological explanation of the words in Aristoph. *Equites*, 546:—

αἴρεσθ' αὐτῷ πολὺ τὸ ῥόθιον, παραπέμψατ' ἐφ' ἕνδεκα κώπαις
θόρυβον χρηστὸν ληναΐτην·

for there were eleven tiers of seats between each *diazoma* of the theatre, which were divided again by the *selis*; so that the spectators

would represent eleven banks of oars, seated, as in the trireme, with the lower rows in advance. In the same way, the use of the *selis* in a trireme, as the gangway for the officers, &c., explains the lines in the *Agamemnon* of Æschylus, 1588–9:—

σὺ ταῦτα φωνεῖς νερτέρᾳ προσήμενος
κώπῃ, κρατούντων τῶν ἐπὶ ζυγῷ δορός;

for if the *zygitæ* had been intended, they must have been described as τῶν ἐπὶ ζυγῶν. The same view of the σέλματα, as the proper place for the officers, was used to explain another passage in the same play (v. 1413), where Agamemnon's companion is described as ναυτικῶν σελμάτων ἰστοτριβής. And the risk of passing along these planks, with intervals between them, was considered to explain the proverbial warning that we must take care not to miss our footing and fall into the hold (Eurip. *Heracl.* 168). Other points were incidentally noticed.

November 10, 1856.

The Master of Trinity read a paper "On the Platonic Theory of Ideas."

In this, he first stated the Platonic theory of ideas as given by the late Professor Butler of Dublin, in his 'Lectures' (vol. ii. p. 117); he then remarked that this theory had evidently, for one of its objects, to explain the possibility of necessary, and therefore eternal truths; and thus was an attempt to solve the problem, often debated in modern times, of the grounds of mathematical truth; an attempt especially called out by the Heraclitean skepticism of Plato's time. The doctrine of ideas which belong to the *intelligible*, not to the visible *world*, and which are the basis of demonstration, did really answer its purpose, and account for the existence of real and eternal truths; and at the same time, by the tenet that sensible things participate in those ideas, accounted for the securing of truth respecting the sensible world. But when Plato goes on to speak of ideas of tables and chairs and the like, he gives an extension of the theory which solves no difficulty, and for which no valid reason is rendered.

The arguments against this extension of the theory are given with great force in the Dialogue entitled *Parmenides*, and are not answered there, nor in a satisfactory way, in any part of Plato's writings. Moreover, throughout this Dialogue, Parmenides is represented as having, in his conversation with Socrates, vastly the superiority, not only in argument, but in temper and manner; and Socrates and his friends, after a little show of resistance, assent submissively to all that Parmenides says. On this ground the writer maintains that the Dialogue is not Plato's, but anti-Platonic, written probably by an admirer of Parmenides, and tending to represent Socrates and his disciples as poor philosophers, conceited talkers, and feeble disputants.

This view was further confirmed by arguments drawn from the external circumstances of the Dialogue.

November 24, 1856.

An account was given by Professor Miller, of the restoration of the Standard of Weight (*vide* Proceedings of Royal Society, vol. viii. Nos. 21, 22).

December 8, 1856.

Mr. Humphry read a paper "On the relations of the Vertebrate Skeleton to the Nervous System."

He pointed out that the central parts, both of the skeleton and of the nervous system, are composed of segments placed in front of, or above one another: those of the former being called "vertebræ," those of the latter "ganglia;" that the vertebræ correspond with the ganglia, each vertebra having its appropriate ganglion; and further, that the processes, or nerves, emanating from the central ganglionic portion of the nervous system correspond with, and accompany the processes, or bones, appended to the central portion of the skeleton, so that the bones appended to any particular vertebra are generally accompanied by the nerves emanating from the ganglion connected with that vertebra. Hence, where a difficulty is found in referring a bone to its vertebra, assistance may often be derived from a reference to the nerve or nerves which accompany that bone. Following the guide thus indicated, Mr. Humphry would refer the upper extremity, not to one—the occipital—vertebra, according to the plan of Professor Owen, but to several cervical vertebræ, forasmuch as it derives its nerves from a considerable tract of the cervical portion of the cord. For the like reason, the lower extremity may be regarded as appertaining to several lumbar and sacral vertebræ. The relations of the bones of the face to their respective cranial vertebræ were pointed out in accordance with the distribution of the cerebral nerves. It was shown, that although the size and shape of the skull are proportioned to the size and shape of the brain, yet that, as a general rule, the thickness and weight of the skull are in an inverse ratio to the size of the brain. A comparison of the different nations of mankind proves, moreover, that the size of the whole skeleton, as well as that of the skull, is usually proportioned to the size of the brain; a well-developed *physique* being the natural associate of an ample *cerebrum*.

PROCEEDINGS

OF THE

CAMBRIDGE PHILOSOPHICAL SOCIETY.

February 9, 1857.

Prof. Challis gave an account of his Observation of the Occultation of Jupiter on Jan. 2, 1857.

February 23, 1857.

A paper was read "On the Theory of Polarized Fasciculi, commonly known as Haidinger's Brushes." By the Rev. J. Power, M.A., Librarian of the University.

In this paper the view taken of the subject is similar to that which had occurred to M. Jamin, and which will be found in Poggendorff's *Annalen*, 1849, p. 145, and in the *Comptes Rendus*, tome xxvi. p. 197. The author arrived, however, at the present theory quite independently in the course of last summer, and before he had acquainted himself with the literature of the subject. M. Jamin had taken as an *essai de calcul* the particular semi-visual angle 20°, which lies far beyond the limits within which the phænomenon is visible; and he has not attempted to give the general law for small angles, which was the real problem to be solved.

This is what the author has attempted in the present communication, availing himself of the experimental researches of Chossat given in the *Bulletin de la Soc. Philomatique*, 1818, p. 94.

The subject was rendered more complicated by the circumstance that the formulæ for the intensities of the refracted pencils are given differently by Neumann, Airy, and the author of this paper. Instead of taking any one set of formulæ, the author managed to take them all into consideration by previously showing that Airy's formulæ result from Neumann's by multiplying them by $\frac{\tan\theta_{\prime}}{\tan\theta}$, which is equivalent to $\frac{1}{\mu}\cdot\frac{\cos\theta}{\cos\theta_{\prime}}$; while his own result from the same by multi-

plying them by $\frac{\cos\theta}{\cos\theta_{\prime}}$; θ and $\theta_{\prime}$ being the angles of incidence and refraction.

It follows from thence that, whichever set of formulæ we may prefer, we shall have after refraction, in all cases, the following simple relation for two oppositely polarized incident pencils of equal intensity; namely,

$$\frac{\text{intensity of pencil polarized in the plane of incidence}}{\text{intensity of the pencil polarized at right angles}}$$

$=\cos^2(\theta-\theta_{\prime})$.

This fraction decreases as the deviation $\theta-\theta_{\prime}$ increases; it is therefore less for the violet rays than the red, for the indigo than for the yellow; and this serves to explain in a general way the dingy yellow stripe in the plane of polarization, and the bright violet stripe in the plane at right angles.

The author has also considered the effect in a plane making an angle ϕ with the plane of greatest polarization, and arrives at the following result: that, provided we attribute to the distribution of the optic nerve such a variation of sensibility as, taken in conjunction with the action of the iris, shall produce a field of view uniformly bright from the centre outwards when common daylight is viewed (a condition which the author believes is common to all eyes with his own), we shall have for the brightness at any point of the field of view the following expression,

$$M.\left(1-e\gamma\frac{\theta^2}{2}\cos 2\phi\right),$$

where M is the central brightness, e the degree of polarization (being 0 for common daylight, and 1 for completely polarized light), and $\gamma=\cdot 07309$ for rays of mean refrangibility.

The last expression gives us without difficulty the form of the curves of equal brightness.

Assuming this constant brightness to be c M, and putting

$$x=\theta\cos\phi,$$

$$y=\theta\sin\phi,$$

we find for the equation sought

$$x^2-y^2=\frac{2(1-c)}{e\gamma}$$

The curves are therefore equilateral hyperbolas having the lines in octants for their common asymptotes, which confound themselves with the curves themselves when $c=0$, the case of mean brightness.

The yellow fasciculi have their vertices in the plane of polarization, and the violet fasciculi have their vertices in the plane at right angles.

It will be seen that for a given value of θ, the brightness, for rays

of all degrees of refrangibility, that is for all values of γ, is least in the plane of greatest polarization and greatest in the plane at right angles—contrary to the idea of Moigno, who, for insufficient reasons, imagined that the maximum occurred in the plane of polarization and the minimum in the plane at right angles.

The yellow tint in the position of minimum intensity, and the violet tint in the position of maximum intensity, is nevertheless perfectly accounted for by the consideration that γ is greater for the violet and indigo rays than for the red and yellow.

The paper further contains some observations respecting a subjective centre of the eye, distinct from the usual objective centre, which may be read with interest, as they remove some difficulties connected with the theory of vision, which had often occurred to the author, and may have occurred to others.

March 9, 1857.

Mr. Hopkins gave an account of some experiments on the conductivity of various substances, and pointed out the bearing of the results on theories of terrestrial heat.

April 27, 1857.

Mr. Humphry read a paper "On the Proportions of the Human Frame."

May 11, 1857.

A paper was read by Professor Stokes, "On the Discontinuity of Arbitrary Constants which appear in Divergent Developments."

In a paper "On the Numerical Calculation of a class of Definite Integrals and Infinite Series" printed in the ninth volume of the 'Cambridge Philosophical Transactions,' the author succeeded in putting the integral $\int_0^\infty \cos\frac{\pi}{2}(w^3-mw)dw$ under a form which admits of receiving every numerical calculation when m is large, whether positive or negative. The integral is obtained in the first instance under the form of circular functions for m positive, or an exponential for m negative, multiplied by series according to descending powers of m. These series, which are at first convergent, though ultimately divergent, have arbitrary constants as coefficients, the determination of which is all that remains to complete the process. From the nature of the series, which are applicable only when m is large, or when it is an imaginary quantity with a large modulus, the passage from a large positive to a large negative value of m cannot be made

through zero, but only by making m imaginary and altering its amplitude by π. The author succeeded in determining directly the arbitrary constants for m positive, but not for m negative. It was found that if, in the analytical expression applicable in the case of m positive, $-m$ were written for m, the result would become correct on throwing away the part involving an exponential with a positive index. There was nothing however to show *à priori* that this process was legitimate, nor, if it were, at what value of the amplitude of m a change in the analytical expression ought to be made, although the occurrence of radicals in the descending and ultimately divergent series, which did not occur in ascending convergent series by which the function might always be expressed, showed that some change analogous to the change of sign of a radical ought to be made in passing through some values of the amplitude of the variable m. The method which the author applied to this function is of very general application, but is subject throughout to the same difficulty.

In the present paper the author has resumed the subject, and has pointed out the character by which the liability to discontinuity in the arbitrary constants may be ascertained, which consists in this, that the terms of an associated divergent series come to be regularly positive. It is thus found that, notwithstanding the discontinuity, the complete integrals, by means of divergent series, of the differential equations which the functions treated of satisfy, are expressed in such a manner as to involve only as many unknown constants as correspond to the degree of the equation.

Divergent series are usually divided into two classes, according as the terms are regularly positive, or alternately positive and negative. But according to the view here taken, series of the former kind appear as singularities of the general case of divergent series proceeding according to powers of an imaginary variable, as indeterminate forms in passing through which a discontinuity of analytical expression takes place, analogous to a change of sign of a radical.

A communication was likewise made by the Rev. W. T. Kingsley, "On the application of Photography to Wood Engraving."

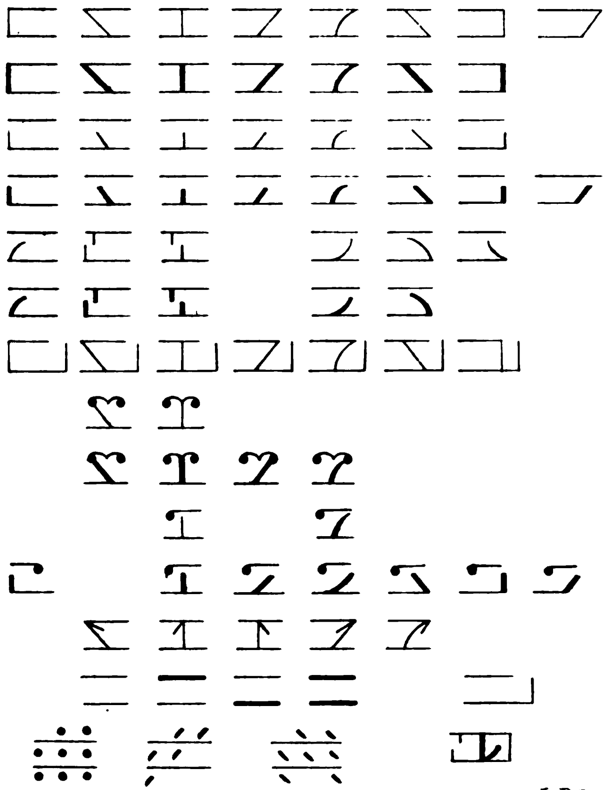
May 25, 1857.

Mr. Bashforth made a communication "On some Calculations and Experiments undertaken for the purpose of testing the Theories of Capillary Action."

Also Mr. Candy exhibited a Physiological Alphabet.

The principle of this alphabet is to make the form of the letter indicate the manner in which the sound is produced, by showing the position of the organs of speech concerned.

CANDY'S PHYSIOLOGICAL ALPHABET.



J. Palmer, Camb.

The open mouth, affording an uninterrupted, unimpeded passage to the breath, is represented by two horizontal straight lines (=).

The sudden, complete, closing of the lips, which gives the sound of the letter P, is represented by drawing a line joining these two on the left ([); the mouth looking in the same direction as the Queen's head on the coins and stamps.

All the letters in the first *column* of the Table have a line on the left, and are therefore labials.

The second column, having a line inclining to the left, are dentals.

The third column have a vertical line in the centre of the mouth, and are the linguals.

The fourth column incline to the right, and are the palato-linguals or weakened linguals.

The fifth column, with a line curving over to the right, are the Sanscrit cerebrals, in which the tongue is curled up, so that the lower side of it comes against the roof of the mouth.

The sixth column, in which the line leans from the right to the centre, are the palatals.

The seventh column, with a vertical line on the right, are the gutturals, at the back of the mouth.

The eighth column, with a line on the right, leaning to the right, are the faucals, still further back than the gutturals.

The first *row* of letters in the Table, with a thin line from top to bottom, are the mute explodents, in which there is a sudden and complete stoppage of the breath.

The second row, with a thick line, are the corresponding sonant explodents.

The third row, in which there is an interval between the top line and the connecting line, are the whispered continuants, in which there is a passage of breath, producing a hissing.

The fourth row, with a short thick line, are the corresponding voiced continuants, or buzzes.

The fifth row contains additional hissing letters, and the sixth row corresponding additional buzzing ones.

In the letters of the seventh row, there is represented an opening upwards at the back of the mouth, leading into the nose. These, therefore, are the nasal letters.

The double curl in the eighth and ninth rows indicates a vibration of both sides of the tongue, while the tip is fixed. This is the case in L, which is represented in Sanscrit by a similar form. The eighth row, with thin lines, are the whispered Ls.

The tenth and eleventh rows, with a single curl, are the Rs or trills. The curl indicates vibration.

The twelfth row are the Caffre clicks. The short line darting out from an angle, shows that the tongue is placed in a certain position, and then suddenly jerked away.

The thirteenth row are the breathings.

The dots and accents (·′‵) in the last row are the vowel-points.

The last figure shows the applicability of this alphabet to monograms, being a combination of F, DZh=J, and K=C, the initials of the inventor, F. J. Candy.

The analysis of sound exhibited in this alphabet is that in 'Universal Writing and Printing,' by Mr. A. J. Ellis, a Fellow of this Society.

The following is Mr. Ellis's Universal Digraphic Alphabet arranged in the order of this alphabet, to facilitate comparison.

p	*tt*	*t*	*tj*	*ț*	*kj*	*k*	*q*
b	*dd*	*d*	*dj*	*d̦*	*gj*	*g*	
ph	*ss*	*s*	*sj*	*sh̦*	*ch*	*kh*	
bh	*zz*	*z*	*zj*	*zh̦*	*jh*	*gh*	ع
wh	*f*	*th*		*sh*	*yh*	*x*	
w	*v*	*dh*		*zh*	*y*		
m	*nn*	*n*	*nj*	*n̦*	*ngj*	*ng*	
	llh	*lh*					
	ll	*l*	*lj*	*l̦*			
		rh		*r̦*			
brh		*r*	*rj*	*zrh*	*ɹ*	ʀ	*grh*
	cc	*c*	*ck*	*cj*	*c̦*		
	ʔ	;	ʜ	ʜ*h*		*a*	

Key to the Consonants and Breathings.

[See the Table for the forms of the Physiological consonants.]

Row 1.
- *p* P, π, פּ with dagesh.
- *tt* Arabic dental *t*, Hebrew ט.
- *t* T, τ, תּ with dagesh.
- *tj* like *ty*.
- *ț* Sanscrit cerebral *t*.
- *kj* French *qu*, Italian *chi*.
- *k* K, κ, כּ with dagesh.
- *q* Hebrew ק.

2.
- *b* B, בּ with dagesh.
- *dd* Arabic dental *d*.
- *d* D, דּ with dagesh.
- *dj* Hungarian *gy*.
- *d̦* Sanscrit cerebral *d*.
- *gj* Old Engl. *gu*ard, French *gu*eux.
- *g* G, גּ with dagesh.

3.
- *ph* φ, פ without dagesh.
- *ss* Arabic dental *s*, Hebrew צ.
- *s* S, σ, ס.
- *sj* Polish *ś*, Hebrew שׂ.
- *sh̦* Sanscrit cerebral *sh*.
- *ch* German palatal *ch* in "ich."
- *kh* German guttural *ch* in "ach!"

4.	*bh*	German *w*, ב without dagesh.
	zz	Arabic dental *z*.
	z	Z, ζ, ז.
	zj	Polish *ź*.
	zh	buzz of the hiss *sh*.
	jh	German palatal *g* in "teig."
	gh	German guttural *g* in "tag."
	ع	Hebrew ע, Arabic ع.
5.	*wh*	whispered *w*, English *wh*, Saxon *hw*.
	f	F: in which the lower lip touches the upper teeth.
	th	θ, ת without dagesh.
	sh	ש, English *sh*, French *ch*, German *sch*.
	yh	whispered *y* in "hue."
	x	Spanish *x* or *j*.
6.	*w*	English *w*, Hebrew ו.
	v	English *v*.
	dh	*th* in "the," ד without dagesh.
	zh	French *j*.
	y	English *y*, German *j*, Hebrew י.
7.	*m*	M, μ, מ.
	nn	Arabic dental *n*.
	n	N, ν, נ.
	nj	Fr. and Ital. *gn*, Span. *ñ*, Port. *nh*.
	n	Sanscrit cerebral *n*.
	ngj	back palatal *n*.
	ng	*ng* in "sing," γ before gutturals.
8.	*llh*	whispered Polish *l*.
	lh	whispered *l*, Welsh *ll*.
9.	*ll*	Polish barred *l*.
	l	L, λ, ל.
	lj	Ital. *gli*, Span. *ll*, Port. *lh*.
	l	Sanscrit cerebral *l*.
10.	*rh*	whispered *r*, Welsh *rh*.
	r	Sanscrit cerebral *r*.
11.	*brh*	German lip-trill.
	r	initial R, ρ, ר.
	rj	palatal *r*.
	zrh	Polish *rz*.
	ɹ	English *r* in "fir," "her."
	ʀ	English *r* in "fur," "poor."
	grh	Arabic غ.

12. *cc* dental click, African *q*.
c flat click, African *c*.
ck side click, African *x*.
cj palatal click, African *qc*.
c cerebral click.

ʔ, Hebrew א, Greek '.

; Arabic Hamza.

H H ‘ ה.

Hh strong *h*, Hebrew ח.

a Sanscrit anuswara, mark of nasalization.

Vowels.

There are three *series* of vowels; the upper, middle, and lower; the points representing which are placed respectively above, between, and below the two horizontal lines of the letter.

There are also three *positions* of vowels; labial and dental (which may be called front), palatal, and guttural. The points are placed respectively at the left hand, middle, and right of the letter.

In some cases there are three vowels of the same series in the same position. These are expressed respectively by · / \ in the appropriate place.

The number of vowels which may be expressed is thus twenty-seven; but there are only twenty distinct vowels recognized, of which ten occur in English. There are no front upper vowels.

Each of these twenty vowels may be long or short. The long vowels may be expressed by doubling, or thickening, the sign for the short vowel; or perhaps better by combining with it the mark (–), giving –, ∠ or <, ⦣ or > for the long sounds of (·) (/) (\) respectively.

Scale of Vowels.

Position :—	Front.	Palatal.	Guttural.	Series.
		Short.		
Candy's Physiol. Alphabet.		\ / ·	\ / ·	Upper.
	\ /	\ / ·	\ ·	Middle.
	\ /	·	\ ·	Lower.
Ellis's Digraphic Alphabet.		*i iə e*	*eə ae a*	Upper.
	ue uh ih	*eh oh eo*	*oe ə*	Middle.
	u uə o	*oə*	*ao aə*	Lower.
		Long.		
Candy's Physiol. Alphabet.		> < –	> < –	Upper.
	> < –	> < –	> –	Middle.
	> < –	–	> –	Lower.
Ellis's Digraphic Alphabet.		*ii iiə ee*	*eeə aae aa*	Upper.
	uue uuh iih	*eeh ooh eeo*	*ooe əə*	Middle.
	uu uuə oo	*ooə*	*aao aaə*	Lower.

Key to the Vowels.

Ph. Digr. *Short.*		Phys. Digr. *Long.*	
a,	Ital. and Germ. short *a*.	*aa*,	*a* in "father."
ae,	English short *a*.	*aae*,	Provincial English.
eə,	English short *e*.	*eeə*,	*ea* in "bear."
e,	French *é*.	*ee*,	English *ay*, French *ée*.
iə,	English short *i*.	*iiə*,	the long sound of
i,	French short *i*.	*ii*,	Engl. *ee*, Fr. *î*, Germ. *ie*.
ə,	Engl. & Dutch *u*, Sanscr. *a*.	*əə*,	the long sound of
oe,	Fr. *eu*, Ger. *oe* or *ö* short.	*ooe*,	Fr. *eû*, Germ. *oe* or *ö* long.
eo,	French mute *e*.	*eeo*,	the long sound of
oh,	Gaelic *ao* in "laogh."	*ooh*,	the long sound of
eh,	Polish *y* short.	*eeh*,	the long sound of
ih,	Welsh *y* (tongue between [teeth)	*iih*,	Welsh *y* long.
uh,	Swedish *u* short.	*uuh*,	Swedish *u* long.
ue,	Sc. *ui*, Ger. *ue* or *ü*, Sw. *y*	*uue*,	German *ue* or *ü* long.
aə,	French *a* short.	*aaə*,	French *â*.
ao,	English *o* short.	*aao*,	English *a* in "water."
oə,	Italian *o* aperto, short.	*ooə*,	Ital. *o* aperto, long.
o,	English *o* in "omit."	*oo*,	Engl. long *o*, Gr. *ω*.
uə,	Ital. *o* chiuso, Sw. *o* short	*uuə*,	Ital. *o* chiuso, Sw. *o* long.
u,	Engl. *oo* short; Germ. &c. *u* short.	*uu*,	Engl. *oo* long, Germ *uh*.

November 9, 1857.

Professor Sedgwick gave a description of a series of dislocations which have moved the Cambrian and Silurian rocks between Leven Sands and Duddon Sands, several miles out of their normal position in the Geological Map of the Lake Mountains.

To make the subsequent descriptions clear, the author first gave a normal or typical section of the older Palæozoic rocks, by enumerating (in an ascending order) the great groups which have been well established, as follows:—1. Skiddaw Slate. 2. Chloritic slate, porphyry, trappean shales, &c. 3. Coniston Limestone, calcareous

slate, Coniston Flagstone, &c. (All the above three groups are called Cambrian.) 4. Coniston Grit,—a good physical group, which however, from the paucity of its fossils, may be thought of ambiguous relations: it appears to represent the May Hill Sandstone, and therefore to be the base of those rocks which, in the north of England, represent the Wenlock and Ludlow groups of Siluria. 5. A coarse slate, often much contorted. 6. A bed of impure limestone, which may be traced from Tottlebank Fell towards the north-east for about two miles, after which it thins out and does not again appear further northward. 7. A coarse and often contorted state. When No. 6 is wanting, No. 5 and No. 7 may be considered as one group (now called Banisdale Slate), which is widely spread, and gradually passes into more coarse and gritty masses, that link themselves to the next superior group. 8. Grit, slate, and tilestone, expanded between Kendal and Kirkby Lonsdale. This is followed by unconformable masses of Old Red Sandstone and carboniferous limestone. Of the above groups, Nos. 5, 6, and 7 approximately represent the Wenlock series, and No. 8 abounds in characteristic Ludlow fossils.

In the range of these groups from Shap Fell to Tottlebank Fell (which is about two miles south-west of Coniston Waterfoot) there seems to be no ambiguity; but to the south of Tottlebank Fell the groups have, through the intervention of great *faults*, been thrown into such abnormal positions that their relations have often been misunderstood. Thus, (in 1822) when the author first attempted to map this part of Lancashire, he was led, by the line of strike as well as by the whole physical characters of the country, to identify the Tottlebank Limestone with a calcareous band a few miles further south, which ranges (on the east side of the Duddon estuary) from the hills above Bank House, through Meer Beck, towards the village of Ireleth, and which from thence by an enormous *fault* (*upcast* towards the south) is thrown into the ridge of High Haume, near Dalton, from which it is continued nearly in the same strike till it is covered by the Old Red Sandstone and Mountain Limestone. This identification was however erroneous; for the limestone-beds above mentioned, ranging on the east side of Duddon Sands, are altogether unconnected with the Tottlebank Limestone, and are in mineral type and fossils absolutely identical with that portion of the Coniston Limestone which appears, as is well known, on the other side of Duddon Sands in the south-western extremity of Cumberland.

The above mistake (made in like manner by several subsequent observers) was partially corrected by the author in 1845; when, on good physical and fossil evidence, he placed the High Haume Limestone on the same parallel with that of Coniston. Not having any fossils from the limestone quarries north of the village of Ireleth, and not having found a single characteristic fossil from the slate-rocks between Ulverston Sands and Duddon Sands, he was in 1845 unable to carry his correction any farther; but even then he remarked again and again that the calcareous beds north of the village of Ireleth in structure resembled the Coniston Limestone, which is seen on the north-west side of the Duddon, much more nearly than they resembled the calcareous beds of Tottlebank.

In the autumn of 1856 the author, accompanied by his friends (Mr. Gough of Reston Hall, and Mr. John Ruthven of Kendal), saw for the first time the excellent local collection formed during the labours of many past years by Mr. Bolton of Ulverston. It was evident almost at a glance that the fossils he had collected from the slate-rocks between Ulverston and Duddon Sands belonged to the upper part of the Coniston group (No. 3). He kindly pointed out to them some of his best localities, and they left the country convinced that nearly all the older rocks between Ulverston and the Duddon estuary belonged to the Coniston group, and consequently that these rocks were not superior, but inferior to the Coniston grits, though the prevailing dip and the geographical position of the groups might seem to indicate the very contrary.

In 1857 they again visited the district, re-examined Mr. Bolton's unrivalled local collection, and again during two days made traverses under his guidance. They then devoted a few days to the approximate determination of the vast breaks and faults, which have so much disturbed the normal position of the physical groups in a part of Furness, and made the colours of the Geological Map to appear almost incredibly anomalous.

The author then described, by help of plans and sections, the *faults* above alluded to. 1. Black Coomb, protruded as it is at the south-west end of Cumberland, seems to have been a kind of centre of disturbance. The chloritic slates and Coniston group which skirt the south-east side of Black Coomb have been ripped up by a north and south *fault* which at one cast throws the Coniston limestone about three miles to the south of its previous range.

2. Similar enormous *up-casts* towards the south-east cause the repetition of the Coniston limestone and flagstone on the other side of the Duddon Sands. This repetition is not produced by undulations, but by great *up-cast faults*.

3. A great east and west *fault* descends near the rivulet of Beck Side with a *down-cast* to the north, which brings the Ireleth slates down to the level of the sea at Sandside.

4. A complicated fault, or system of faults, with a very great *up-cast* to the south-east, runs from Kirkby Hall, skirting the brow of the hill under the great Ireleth slate quarries. The slates of Ireleth cannot be separated from the Coniston flag. They do not overlie the Coniston grits (as the author and other observers had long supposed), but abut against them. This conclusion seems inevitable, though the sections are broken and difficult of interpretation.

5. By a complication of *faults* the Coniston grits are widely expanded in the hills immediately north of the great Ireleth slate quarries; but all the above mentioned groups are by an east and west *fault* (or series of faults), with an enormous up-cast towards the south, cut off from the normal groups (viz. Nos. 4, 5, 6, and 7 of the typical section), which range towards Coniston water and thence into Westmoreland. This east and west fault runs down into the valley of the Crake, not far from Lowick Bridge.

6. Another great *fault* appears to descend from Coniston water-

head down the Crake, producing an up-cast on its south-east side.

The facts given in the above abstract necessitate a partial change of nomenclature. The Ireleth slates can no longer be appealed to as groups *superior* to the Coniston grits and on the parallel of the Wenlock shale. But in Banisdale there are old slate quarries in the group which, without sectional difficulty or ambiguity, does overlie the Coniston grits. For the future, therefore, the author proposes to use the term Banisdale slates for the lower part of the group which overlies the Coniston grits.

Having approximately laid down the *faults* above mentioned, there was still something wanting to complete the evidence; for the slate rocks between Duddon Sands and Leven Sands, in spite of their contortions, form an ascending section of great thickness. If, therefore, these slates be a repetition of the Coniston flags, the Coniston grits (typical section No. 4) might be looked for somewhere towards the south-east. To put this to the test, the author (accompanied by his two friends) made a complete traverse from Broughton to the upper part of Leven Sands; and they found, as they were finishing their traverse, that the ridges which skirt the estuary of the Leven, below Penny Bridge, were composed of the Coniston grits in their characteristic form. The evidence was then complete, and they next day left the country.

Also a paper was read by Professor De Morgan, "On the Beats of Imperfect Consonances."

This subject has been left in great obscurity by Dr. Smith, and subsequent writers have either neglected it, or misunderstood it, or obtained results by methods which miss the principal simplification of which the theory appears susceptible. Omitting historical matter, Mr. De Morgan's method may be described as follows:—

The grave harmonic of Tartini, formed by sounding two notes of which the vibrations take n and m equal parts of time ($m : n$ being in its lowest terms), has a vibration which lasts through mn of those times. This is called *Tartini's beat*, whether it produce a sound, or whether it only produce what Dr. Smith calls a *fluttering*. This beat is most perfect when the consonance is in perfect tune. If the consonance be a little out of tune, Tartini's beats are not destroyed, but do not succeed each other with perfect reiteration of circumstances, owing to the gradual advance or regression of the position in one vibration of the commencement of the other. A cycle of disturbances is the result, which cycle is repeated, or repeated *quam proxime*; and the ear recognizes this recurrence in *Smith's beats*, which are entirely due to the imperfection of the consonance. The connexion has a close resemblance to that of the instantaneous ellipse of a planet and its disturbed orbit.

The simplest connexion of beats and vibrations is as follows:— The smaller of the two numbers, n and m, being n, every vibration by which the upper note is tuned wrong gives n beats per second. Thus, the consonance being a fifth (2 : 3), every vibration by

which the upper note is too flat or too sharp gives two beats per second.

In an appendix, Mr. De Morgan gives some tables of beats, repeats some theorems on temperament from the Penny Cyclopædia, and recommends and argues in favour of tuning being performed by a *whole octave* of tuning forks, adjusted by beats to the system employed.

November 23, 1857.

A paper was read by Professor Thompson, "On the *Sophista* of Plato."

In this paper the genuineness of the *Sophista* was defended, and some of its philosophical bearings pointed out. In answer to the doubts expressed by the Master of Trinity in a previous communication, it was shown that the *Sophista*, as well as the *Politicus*, which is a continuation of it, are repeatedly referred to in the works of Aristotle. In particular, *Arist. Metaph.* v. ii. 93 was appealed to as evidence that Aristotle had not only read the dialogue called *Sophista*, but believed it to have been written by his master.

The Dialogue was analysed, and shown to be a critique of the negative or Eristic systems of logic, derived from the Eleatics, which were taught by Euclides and Antisthenes, the founders of the Socratic sects of the Megarics and Cynics respectively. Many allusions, personal and otherwise, to Antisthenes were pointed out, as existing both in this dialogue and in the *Theætetus*, of which it is a professed continuation. The *Theætetus* was regarded as a critique of the contemporary psychology, and the *Sophista* as a confutation of the prevailing schemes of logic; and both were shown to contain exemplifications of the twin processes of Induction or Collection, and Division or Classification, which constitute, according to Plato in the *Phædrus*, p. 265 E., the science or art of Dialectic.

It was also argued, in opposition to Schleiermacher, that the Materialistic doctrines confuted in *Sophista*, p. 246, represent those of Antisthenes, rather than the atomic theory of Democritus, or the empirical system of Aristippus.

The analysis of the simple Proposition (*Soph.* p. 262) was shown, by the testimony of Plutarch and others, to be Platonic, and the imperfect Idealists refuted in p. 276 were identified with the Megarics, and distinguished from the Platonists.

Passages were also quoted from the *Politicus*, showing the disciplinary and educational uses to which the method of Division was made subservient in the teaching of the Academy; and this teaching was further illustrated by a quotation of considerable length from a Comic Poet *ap. Athen.* lib. ii.

Incidentally, Porphyry and Abelard were appealed to in evidence that Plato's Method of Division was known to the Neo-Platonists and the Schoolmen, and recognized by them as characteristic of his Dialectic.

December 7, 1857.

Professor Miller made a communication on the Planimeters of Wetli, Decher, and Amsler, and communicated the following simple proof of the principle of Amsler's, due to Mr. Adams.

Let O be the fixed point,

P the tracer,

Q the hinge,

W the centre of wheel,

M the middle point of P Q,

$OQ=a$, $PQ=b$, $MW=c$.

The area of any closed figure whose boundary is traced out by P, is the algebraical sum of the elementary areas swept out by the broken line O Q P in its successive positions.

Let ϕ and ψ be the angles which OQ, QP at any time make respectively with their initial positions.

s the arc which the wheel has turned through at the same time.

If now OQP take up a consecutive position, and ϕ, ψ, s receive the small increments $\delta\phi$, $\delta\psi$, δs, we see that $\delta s =$ motion of W in direction perpendicular to PQ.

Hence motion of M in the same direction $=\delta s+c\delta\psi$, and therefore the elementary area traced out by $QP=b(\delta s+c\delta\psi)$. Also elementary area traced out by $OQ=\frac{1}{2}a^2\delta\phi$.

Hence the whole area swept out by OQP in moving from its initial to any other position is

$$\tfrac{1}{2}a^2\phi+bc\psi+bs.$$

If OQP returns to its initial position without performing a complete revolution about O, the limits of ϕ and ψ are 0, and the area of the figure traced out by P is bs.

If OQP has performed a complete revolution, the limits of ϕ and ψ are 2π, and the area traced out is

$$\pi(a^2+2bc)+bs.$$

A paper was also read by the Astronomer Royal, "On the substitution of Methods founded on Ordinary Geometry for Methods based on the General Doctrine of Proportions, in the treatment of some Geometrical Problems."

The doctrine of proportions laid down in the fifth book of Euclid is the only one applicable to all cases without exception, but it is cumbrous and difficult to remember. It is therefore natural to attempt, in special applications of the doctrine, to introduce the facilities which are special to each case. This has been done long

since in the case of numbers, and this the author of this paper attempts in some cases in which geometrical lines only are the subject of consideration, by a new treatment of a theorem equivalent to Euclid's simple *ex æquali* and of the doctrine of similar triangles, referring to nothing more advanced than Euclid, Book II.

The author proves,—

1. If the rectangle contained under the sides a, B be equal to the rectangle contained under the sides b, A; and if these rectangles be so applied together that the sides a and b shall be in a straight line and that the side B shall meet the side A, the two rectangles will be the complements of the rectangles on the diameter of a rectangle.

2. If the rectangle contained under the lines a, B is equal to the rectangle contained under the lines b, A; and if the rectangle under the lines b, C is equal to the rectangle contained under the lines c, B; then will the rectangle contained under the lines a, C be equal to the rectangle contained under the lines c, A.

(This is equivalent to the ordinary *ex æquali* theorem.

If $\quad a : b :: \mathrm{A} : \mathrm{B}$

and $\quad b : c :: \mathrm{B} : \mathrm{C}$,

then will $\quad a : c :: \mathrm{A} : \mathrm{C}$.)

3. If two right-angled triangles are equiangular, and if a, A are their hypothenuses, and b, B homonymous sides, the rectangle contained under the lines a, B is equal to the rectangle contained under the lines b, A.

(The equivalent theorem in proportions is

$$a : b :: \mathrm{A} : \mathrm{B}.)$$

4. If a, c and A, C are homonymous sides of equiangular triangles, the rectangle contained under a, C will be equal to the rectangle contained under c, A.

5. If b, c and B, C are homonymous sides including the right angles of two equiangular right-angled triangles, the rectangle contained under b, C will be equal to the rectangle contained under c, B.

6. If the rectangle contained under the lines a, B is equal to the rectangle contained under the lines b, A; the parallelogram contained under the lines a, B will be equal to the equiangular parallelogram contained under the lines b, A.

(This is equivalent to the proposition,

If $\quad a : b :: \mathrm{A} : \mathrm{B}$

then $\quad a : b :: \mathrm{A} \cos \alpha : \mathrm{B} \cos \alpha$.)

These propositions will suffice for the treatment of the first thirteen propositions of Euclid's sixth book (Prop. I. excepted), and of all the theorems and problems apparently involving proportions of straight lines (not of areas, &c.) which usually present themselves. The author then proceeds, as an instance of their application, to prove by means of them the following theorem:—

If pairs of tangents are drawn externally to each couple of three unequal circles, the three intersections of the tangents of each pair will be in one straight line.

Also a paper was read by Professor De Morgan, "On a Proof of the existence of a Root in every Algebraic Equation: with an examination and extension of Cauchy's Theorem of Imaginary Roots; and remarks on the proofs of the existence of Roots given by Argand and by Mourey."

The extension of Cauchy's theorem is very easily found, when the proof is the first of those given by Sturm in Liouville's Journal. The extended theorem is as follows:—

Let ϕz be any function of z, and let $z=x+y\sqrt{-1}$. Let (x, y) be a point on any circuit which does not cut itself. Let this point describe the circuit in the positive direction of revolution; and, $\phi(x+y\sqrt{-1})$ being $P+Q.\sqrt{-1}$, let $\frac{P}{Q}$ change sign k times as in $+0-$, and l times as in $-0+$. Let (x, y) be called a radical point when $\phi(x+y\sqrt{-1})=0$, or $=\infty$. Let there be m radical points of the first kind within, and m' upon, the circuit: let there be n radical points of the second kind within, and n' upon, the circuit. Then

$$k-l=2m+m'-(2n+n').$$

Sturm's demonstration of the case where $m'=0, n=0, n'=0$, which is Cauchy's theorem, assumes the existence of the roots of an algebraical expression. Mr. De Morgan's proof of the existence of these roots is as follows:—He shows, *à priori*, that in the sequence of signs which Cauchy's theorem requires to be examined, $k-l$ never undergoes any alteration except after 0 and ∞ have coincided, that is, where $P=0$, $Q=0$, simultaneously. It is then easily proved that change in $k-l$ happens in every algebraical equation.

The proofs given by Argand and Mourey were intended as illustrations of the power of the extension which is now called *double algebra*. Stript of this interpretation, they are purely algebraical, and Argand's proof is really that which was afterwards found by Cauchy. Argand's proof is more simple in form than Cauchy's.

February 8, 1858.

A paper was read by the Rev. O. Fisher, "On the probable origin of numerous Deep Pits on some Heaths in Dorsetshire."

Also a paper was read by Professor De Morgan, "On the Syllogism, No. III., and on Logic in general."

This paper is divided into two sections, the first of which is descriptive and controversial, the second is an abstract of the system.

Between the opinion of Kant that logic cannot be improved, and that of some recent writers, who hold it perverted, and not always correct, the truth is held to lie in this,—that existing logic, in its *quod semper, quod ubique, quod ab omnibus*, is true and accurate; but that it is only a beginning, and that the low estimation in which it has been held is a consequence of its incompleteness.

The modern definition of logic, the *form of thought*, relates to a distinction which is more familiar to mathematicians than to logicians, but is rather in the common use of the mathematician than in his clear apprehension. Aristotle, who first implicitly made the distinction of form and matter, was a mathematician; and so also was Kant, who first explicitly introduced this distinction into the definition of logic. The only two nations who had a logic taking character from the distinction, the Greeks and Hindus, are precisely the two nations to whom we owe the rudiments of our mathematics. It is affirmed by the author, that, in our time, the distinction is more in the theory of the logician than in his practice, more in the practice of the mathematician than in his theory.

Various illustrations are given of the manner in which recent logical writers have, according to Mr. De Morgan, misconceived the distinction of formal and material. In another part of the paper he suggests that this distinction has been confounded with the distinction which he designates as onymatic and non-onymatic. By *onymatic* he means what arises out of the use and meaning of nomenclature: thus the relation of containing and contained is an important relation of names to each other as names, or an onymatic relation.

The modern logic, by the simplicity of its *final* examples, is prevented from being of much use as a mental gymnastic. Instances are given of a proposition and a syllogism which are more worthy of being propounded as *exercises* than the instances which are found in works on logic.

The objections to symbols are discussed. Every science which has thriven has organized symbols of its own: and logic, the only science which confessedly has made no progress for many centuries, is also the only science which has grown no symbols.

The logicians have confined themselves hitherto within what Mr. De Morgan calls the *logico-mathematical* field: they now begin to contend for the inclusion of what he calls the *logico-metaphysical*. This distinction they take as that of *extension* and *comprehension*. The author contends for a distinction of *extension* and *intension* in both the sides of logic, the mathematical and the metaphysical; though undoubtedly extension predominates in the mathematical side, and intension in the metaphysical. These distinctions are onymatic. If the name C contain all that is in A or in B, or in both, symbolized by C=(A, B), then A and B are in the extension of C. But if C be contained both in A and in B, symbolized by C=A-B, or AB, then A and B are in the intension of C.

A name is used in four senses. It is the name of an object, or of a quality inhering in an object, and distinguishing a class: these two

uses are objective. It is also the name of a class, or of an attribute by which the mind thinks of a class: these two uses are subjective. The subjective uses are reductions of plurality to unity, a description for the truth and reality of which the author contends.

It is affirmed that the logicians have not only confined themselves within the mathematical side of logic, but that even the recent attempts to introduce the metaphysical appear like attempts to create a second mathematical branch. This is evidenced by the manner in which unity of *attribution* has been discarded in favour of plurality of *qualification*. Thus it has been said, in obedience to the theory of quantification of the predicate, that the *humanity* of Newton is a different thing from the *humanity* of Leibnitz. That this view, though true, belongs to the *mathematical* side of logic, is contended for and enforced at length.

Aristotle made the distinction which the logicians now recognize as that of extension and comprehension, and which Mr. De Morgan distinguishes as that of mathematical and metaphysical reading, as follows:—In one sense the species is in the genus: in another the genus is in the species. That is, all the species are *aggregants* of the genus: the whole genus is a *component* of the notion of the species.

Recent English logicians of high name have misconceived this distinction to the extent of imagining that by changing 'Every A is some B' into 'Some B is every A,' they make the change alluded to by Aristotle. Mr. De Morgan restores the old distinction, and completely incorporates what was only partially introduced, the *change of quantity* which takes place in passing from the mathematical to the metaphysical reading. Thus 'Every A is B' is in the first reading 'The whole class A is one aggregant of the class B'; and in the second, 'The whole attribute B is one component of the attribute A.'

The limitation of the *universe* of a proposition, made throughout the author's preceding writings, is again contended for.

A proposition is the assertion or denial of a relation between two notions. Relations which are of necessity involved in nomenclature, are called *onymatic*; and these must be first studied. Mr. De Morgan believes that the logicians have described, under the distinction of *formal* and *material*, no more than the distinction of *onymatic* and *non-onymatic*. The mathematical notion of class, and the metaphysical notion of attribute, give four different readings of a proposition:—1. *Logico-mathematical*, class aggregate of class; man contained in animal. 2. *Logico-physical*, attribute predicated of class; *animality* attribute of the class man. 3. *Logico-metaphysical*, attribute component of attribute; animality a component of humanity. 4. *Logico-contraphysical*, attribute subjected to class; humanity only predicable within the class animal.

The logicians confined logical predication to the idea of class contained in class, species in genus. The genus in species, attribute component of attribute, they relegated to metaphysics. Hence their distinction of the *logical* and *metaphysical* whole. The class composed of individuals they called the *mathematical* whole: Mr.

De Morgan calls it the *arithmetical* whole, transferring the word *mathematical* to what was called the logical whole. The common mode of expression, as 'Every A is B,' &c., he considers as speaking the language of the arithmetical whole, though the speaker may attach the idea of either of the other wholes.

Extension predominates in the mathematical whole; intension in the metaphysical. The most usual mode of speech is the *physical*: man is educated a mathematician as to the subject of his proposition, a metaphysician as to the predicate.

The most remarkable point at issue between Mr. De Morgan and the logicians, is in his opposition to their notion of the whole attribute being the *sum* of its components. The difference between aggregation and composition is one of the turning-points of his whole system.

The distinctions above drawn require differences of language to express the relations which enter: the logicians have nothing but the copula *is*. At the outset, however, we have the distinction which is expressed by speaking of relations of *terminal ambiguity* and relations of *terminal precision*:—the first seen in 'A is contained in B,' where it is left unsaid whether or no A fills B; the second seen in the case in which it is implied that A is part *only* of B.

By speaking in the arithmetical whole, the logicians have made a system of syllogism from which the numerical syllogism cannot be excluded. The propositions 'Some As are Bs,' and '50 As are Bs,' are of the same kind: they are both referred to the arithmetical whole. This whole is subordinate to both the mathematical and metaphysical wholes; though more prominent in the first than in the second.

When inclusion and exclusion are opposed to one another, and combined with assertion and denial, the ordinary proposition takes a form in which quantity is but an emergent incident, and not a fundamental mode of discrimination. Thus the propositions A and O are the assertion and denial of the inclusion of class in class; E and I are the assertion and denial of the exclusion of class from class.

The opposition of the two kinds of quantity, extensive and intensive, is not easy and natural, when the word *quantity* is used in metaphysical reading. Mr. De Morgan proposes the word *force* to express quantity in the second case. He finds this word in use. Thus it is sometimes said that a term is or is not used in its complete force, when the meaning is, that all the attributes of which the term is compounded are or are not involved in the use made of the term. This is, according to Mr. De Morgan, one of the cases in which the logical system of the world at large has got beyond that of the logicians.

The *spicular* notation of the former papers is extended: the signs) and] being used to distinguish mathematical and metaphysical reading. Then X))Y signifies that the whole class X is contained in the class Y; X]]Y signifies that the whole attribute Y is a component of the attribute X.

The syllogism is the deduction of a relation from the combination of two others. By distinction of the mathematical and metaphysical,

of the terminally ambiguous and terminally precise, four modes of combination are obtained. Logicians have but the copula *is* for all cases. Mr. De Morgan proposes to use a complete system of *schetical* terms, by which the combination of relations shall be exhibited. Leaving out the cases of terminal precision, which are more complex and less usual, the two kinds of reading under which the common syllogism is included are as follows:—

Terminal Ambiguity. Mathematical reading.

Relation of Class X to Class Y.

The class x is the *contrary* of X, or contains all the rest of the universe.

Proposition.	X—of Y.	Y—of X.	Notation.
Assertion of X contained in Y	Species	Genus	X))Y
Denial of X contained in Y	Exient	Deficient	X(.(Y
Assertion of X excluded from Y	Coexternal	Coexternal	X).(Y
Denial of X excluded from Y	Copartient	Copartient	X()Y
Assertion of x contained in Y	Complement	Complement	X(.)Y
Denial of x contained in Y	Coinadequate	Coinadequate	X)(Y
Assertion of x excluded from Y	Genus	Species	X((Y
Denial of x excluded from Y	Deficient	Exient	X).)Y

Terminal Ambiguity. Metaphysical reading.

Relation of attribute Y to attribute X.

	Y—of X.	X—of Y.	Notation.
Assertion of Y a component of X	Essential	Dependent	X]]Y
Denial of Y a component of X	Non-essential	Independent	X[.[Y
Assertion of Y incompatible with X	Repugnant	Repugnant	X].[Y
Denial of Y incompatible with X	Irrepugnant	Irrepugnant	X[]Y
Assertion of Y a component of x	Alternative	Alternative	X[.]Y
Denial of Y a component of x	Inalternative	Inalternative	X][Y
Assertion of Y incompatible with x	Dependent	Essential	X[[Y
Denial of Y incompatible with x	Independent	Inessential	X].]Y

The extension of the four forms to eight, the notation, &c., are treated in the second paper on syllogism. The two sets contain the same propositions, differently read; and the quantities in the two are different. In the first reading X) and (X denote X taken universally in extension; X(and)X denote X taken particularly. In the second reading]X and X[are universals, X] and [X are particulars. Thus, when we say that the classes X and Y are copartient, or in common language 'some Xs are Ys,' denoted by X()Y, both X and Y have particular *quantity* in extension. In saying this we also say that X and Y, as attributes, are irrepugnant, or not incompatible, denoted by X[]Y. But the intensive *force* of both X and Y is universal; no one attribute of X is repugnant to any one attribute of Y.

The syllogism denoted by X))Y)(Z contains the assertions that X is a genus of Y and Y a coinadequate of Z, (Y and Z not together filling the universe). The conclusion is X)(Z, X is a coinadequate of

Z, and the combination of relations is seen in—Every species of a coinadequate is a coinadequate. In metaphysical reading, we have X]]Y][Z, X is a dependent of Y, Y an inalternative of Z. The conclusion is X][Z, X is an inalternative of Z, and the combination of relations is seen in—The dependent of an inalternative is an inalternative. When the terms become as familiar as genus and species, the axiomatic character of the combination is as clearly manifest as in—Species of species is species. Mr. De Morgan gives the following instance of a good inference which would probably not be seen with ease in its present form, though the phrases are not technical: "We must not say that either bodily strength or meanness is a necessary alternative, for courage and meanness are incompatible, while courage does not depend on bodily strength." And he maintains that the educated world has made considerable advance in the use of relations of attributes, though the logician has nothing but what he calls the arithmetical *abacus* on which to exhibit the process.

Some modern logicians have so completely fallen into the mathematical view of quantity, that there is a school which treats all thought as relation of more and less. Mr. De Morgan opposes this view.

The second part of this paper, being a non-controversial summary of Mr. De Morgan's system, so far as *onymatic* relations are concerned, hardly admits of abstract. Its principal points have been touched on.

In a postscript, such notice is taken of the late Sir W. Hamilton's criticism on Mr. De Morgan's second paper as circumstances require and will allow.

February 22, 1858.

Dr. Donaldson, of Trinity College, read a paper "On the Statue of Solon mentioned by Æschines and Demosthenes."

The object of the author of this paper was to fix the age and subject of a beautiful statue in the *Museo Borbonico* at Naples, which was recovered from the ruins of the theatre at Herculaneum. This statue has generally been regarded as representing Aristides the Just, the son of Lysimachus; and one attempt has been made to show that it is a portrait of Ælius Aristides, the rhetorician, who was born 38 years after the destruction of Herculaneum in A.D. 79. A more plausible hypothesis, supported by great names, considers the statue as a portrait of Æschines. But this rests on a palpable misconception. After refuting these theories, the author undertook to show that the statue was probably a copy of that erected in honour of Solon in the agora at Salamis, and mentioned in a striking manner by Æschines (c. *Timarch.* p. 4) and Demosthenes (*De Fals. Leg.* p. 420). This was argued from the peculiar and distinctive attitude; from the fact that the treatment of the drapery accorded with that belonging to the school of Scopos, and the costume corresponded to that of the epoch (about fifty years before B.C. 343) assigned to the statue of Solon by Demosthenes; and from the suitableness of a statue of Solon, who was an elegiac

poet as well as a legislator, to the place where the statue was found, namely, the theatre at Herculaneum. Attention was also directed to the improbability of a later appropriation of a statue in such a peculiar posture, and Dion Chrysostomus was cited to show that even the Rhodians, who had adopted the practice of altering the inscriptions of honorary statues, abstained from interfering with those which were defined, not only by the name, but by the characteristics of the person represented.

March 8, 1858.

A paper was read by Dr. Paget, "On some Instances of remarkable Defects in the Voluntary Muscles."

Four original cases, in which large and important muscles, such as the pectorals, were wholly absent or in a state of extreme tenuity; the defects either congenital, or existing from early infancy; limited to certain groups of muscles, and unaccompanied with any defect or deformity of the bones. In three of the cases, the effects symmetrical; in the fourth confined to one side of the chest. Enormous development of the calves in one of the cases.

Also a paper was read "On Organic Polarity," by H. F. Baxter, Esq.

The object of the paper is to show the intimate connexion that exists between *organic* force and the ordinary *polar* forces, such as chemical force, for example.

The principal experiments, showing that organic action, viz. *secretion*, is accompanied with the manifestation of current force, have already appeared in the Royal Society's 'Transactions' for the years 1848 and 1852; but in the present communication the author enters more minutely into the resemblance between the actions which take place in the voltaic circle and those that occur during secretion than could be prudently attempted in his previous papers. But whatever view may be entertained in regard to the *origin* of the power in the voltaic circle, whether by mere *contact* or by *chemical action*, the decision of this point is of no importance to the question under consideration; since the *manifestation* of current force *during* voltaic action is allowed both by the *chemical* theorist as well as the *contact* theorist; and if we admit the manifestation of this force (*current force*) to be evidence of polar action in one class of cases, viz. during voltaic action, we are certainly justified in logically concluding that it may be adduced as evidence of polar action in other cases also, viz. *during* organic action as in secretion. Reference is made to Prof. Graham's researches on *osmose*. According to Prof. Graham, *osmose* would appear to be dependent upon *chemical action*, and consequently, should we be disposed to class the phænomena of secretion with those of *osmose*, we should be thus compelled to acknowledge that the act of secretion must be polar in its nature.

The author does not attempt to show in what manner secretion is effected, his great object being to point out what does occur during this act, viz. the manifestation of *polar* action, and consequently it is upon this ground that we may logically infer that the *force* must be POLAR in its nature.

PROCEEDINGS

OF THE

CAMBRIDGE PHILOSOPHICAL SOCIETY.

April 26, 1858.

Professor Challis made a communication "On the Annular Eclipse of the Sun, March 15, 1858."

The Master of Trinity read a paper "On Barrow, and his Academical Times."

May 10, 1858.

Professor Miller made communications—(1) "On an improved Method of finding the position of any face of a Crystal belonging to the Anorthic System." (2) "On the direction of the Axis of a Zone." (3) On a substitute for the Reflective Goniometer."

Mr. Godfray read a paper "On a Chart and Diagram for facilitating Great-Circle Sailing."

It has long been known that in many cases a *very great* saving of distance would be effected by guiding the ship along the arc of a great circle instead of following the rhumb; but the tedious calculations which great-circle sailing requires, and the difficulty of tracing the track on the chart, have hitherto stood in the way of its adoption. The great advantage which Mercator's sailing offers in these respects sufficiently explains the preference given to it, even by those who are fully aware of the longer route it obliges them to follow.

The object of this communication was to show how the same advantages are secured to great-circle sailing by the adoption of a chart on the central or gnomonic projection, which, with the addition of a diagram, solves the problem of this sailing with the same facility as Mercator's chart does for sailing on a rhumb.

The great-circle track becomes a straight line joining the two places; and this being drawn, the various courses and the distances to be run upon each are obtained, as also the distance from the ship to her destination, by a mere inspection of the diagram and without calculation.

The great-circle track is not always practicable, on account of its taking the ship into too high a latitude, where the ice would render it dangerous or impossible to penetrate. When this is the case, the same chart and diagram will, with just as much facility as before, point out that which, under the circumstances, is the shortest route. Some parallel of latitude is fixed upon for the maximum, and the track to be followed will then consist of a portion of that parallel and of the portions of two great circles which are tangents to it,—one passing through the ship, the other through the destination. On this great-circle chart the track will be the two straight lines drawn from the two places, so as to touch the circle of highest latitude and the part of this circle between the points of contact.

The paper explains the construction of the chart and diagram, and illustrates their use by two examples: the first, from the southern extremity of Africa to Perth in Australia, which shows a gain of 204 miles; the other, from 30° S. lat., 18° W. long. to Melbourne, gives a gain of 1130 miles, without going into a higher latitude than 55° south.

A chart and diagram on this principle have been engraved by the Hydrographic Office, Admiralty.

May 24, 1858.

The Master of Trinity read the conclusion of his paper "On Barrow, and his Academical Times."

November 8, 1858.

Professor Challis made a communication "On Donati's Comet."

November 22, 1858.

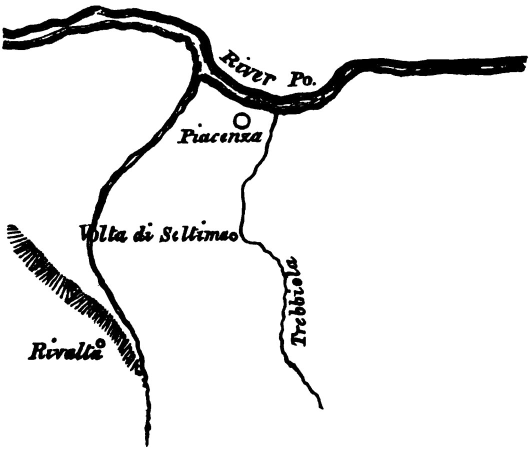
The Public Orator gave a lecture on "The Battle of the Trebbia," the object of which was to compare the different authorities, and to illustrate them by a description of the neighbourhood of Piacenza, which he had recently visited. He showed that Polybius's narrative was the most valuable, as being in all probability the original source of all subsequent accounts, and as deriving an especial interest from the author's personal knowledge of the localities. Livy in the main followed Polybius, amplifying and varying the details rather with a view to an effective and ornamented style than to the actual truth of his statements. Some particulars, however, appeared, in this part of his work as elsewhere, to have been borrowed from L. Cincius Alimentus, or some other historian of the Second Punic

War. Cornelius Nepos, from his brevity, and Appian, from his inaccuracy, were not worthy to be taken into account. No modern historian appeared to have visited the place. The principal points to be determined were (1) the situation of Scipio's camp after he had abandoned his position in the immediate vicinity of Placentia, and (2) the identification of the deep watercourse where Hannibal placed Mago with two thousand men in ambush on the morning of the battle.

Scipio's camp was, beyond doubt, at or near Rivalta, a castle and hamlet situated on a 'high bank' (as the name imports) on the further side of the Trebbia, about nine miles south-west of Piacenza. The ambuscade was placed in the watercourse called the Trebbiola, a small stream, of which the banks were from 6 to 8 feet high, about six miles from Piacenza, above the place called 'La Volta di Settima.'

The passages of Polybius, to which reference was made in the lecture, are in Book III. chapters 66 *sqq.*; those of Livy, in Book XXI. chapters 47, 48, 52–56.

The rude plan given below may make this abstract more intelligible.



December 6, 1858.

"Suggestion of a proof of the Theorem that every Algebraic Equation has a Root." By G. B. Airy, Esq., Astronomer Royal.

In this paper the equation to be discussed is expressed under the form

$$a_\alpha r_\theta^n + b_\beta r_\theta^{n-1} + \ldots + m_\mu = 0,$$

where $a_\alpha = a(\cos\alpha + \sqrt{-1}\sin\alpha)$, $r_\theta = r(\cos\theta + \sqrt{-1}\sin\theta), \ldots$ or,

as it may be written, $P+\sqrt{-1}Q=0$; and the object is to show that there will be at least one value of r between 0 and positive infinity, and one value of θ between 0 and 2π, which, used in combination, will make both P and Q $=0$.

This is effected by constructing two curves whose common abscissa is θ, and whose ordinates are respectively the corresponding values of P and Q, produced by substituting in their expressions the same value of r, and observing the change which takes place in the form of these curves, and in the position of their points of intersection, as r successively assumes all values from 0 to positive infinity. The existence of a root will be indicated by a point of intersection of these curves (the P-curve and Q-curve, as they may be called) falling on the axis of abscissæ. When $r=0$, each of these curves will be a straight line parallel to the axis of abscissæ. When $r=\infty$, the corresponding values of P and Q will generally be indefinitely great; but by reducing their values in the same proportion, which will not affect the validity of the demonstration, the Q-curve will become a line of sines, and the P-curve a line of cosines, or a line of sines drawn back through $\frac{\pi}{2}$. On constructing these curves, which we may call respectively P(0), Q(0), P(∞), (Q ∞), it will be remarked—

(1) That P(0) and Q(0) do not intersect.
(2) That P(∞) and Q(∞) intersect in two points.
(3) That one of these points of intersection is above the line of abscissæ, and the other is below it.

On considering the change in the forms of the P-curve and Q-curve as r increases from 0 to infinity, it will be seen that the P-curve must have intruded on the Q-curve, at first by simple contact; and that, as the intrusion advances, the simple contact is changed into two intersections, which will at first be on the same side of the line of abscissæ. But as, where r is indefinitely increased, any two consecutive intersections necessarily lie on opposite sides of the line of abscissæ, it may be shown, by considering the various ways in which the intrusion may take place, that in all cases one at least of the intersections must have crossed the line of abscissæ during the increase of r; and a root is thus determined.

A communication was also made by Professor Miller "On the contrivances employed by M. Porro in the construction of instruments used in Surveying and Astronomy."

February 14, 1859.

Mr. Humphry made a communication "On the Limbs of Vertebrate Animals."

He gave a brief description of the fore and hind limbs in the

several vertebrate classes, directing attention to the tripartite division of their distal segments, and to the uniformity of plan upon which they are constructed. He argued that this uniformity has relation to the mode in which their development proceeds, and to the similarity of their functions rather than any adhesion to an "ideal archtypal pattern;" and expressed his belief that the existence of such an ideal in the minds of anatomists proves in some measure a hindrance to the full study of the laws which regulate the formation of animal bodies.

The differences between the fore and hind limbs were shown to depend,—*first*, upon the fact that the hind limbs are required to propel as well as to support the trunk, and less variety of movement is needed in them; hence they are larger and firmer, and the same bone of the leg forms a main constituent both of the ankle and of the knee-joint; *secondly*, in walking and running the fore limb is extended in front of the trunk, and draws the latter after it during its flexion, whereas the hind limb is bent up beneath the trunk and drives the latter on before it during its extension. This antagonism in the mode of their action leads to an antagonism in construction of the two limbs,—of their upper segments at least, the posterior aspect of the one corresponding with the anterior aspect of the other. The antagonistic relations are brought about by a partial rotation in the long axis of the two limbs which takes place in opposite directions during development; and coincident with the rotation of the proximal segments of the fore limb in one direction is a rotation of its distal segment in an opposite direction, so as to turn the palm towards the ground.

Reasons were given for regarding the scapular and pelvic arches as formed by modifications of the hæmal, and not of the pleural, parts of the vertebræ; and for believing that the scapular arch belongs, not to the occipital bone, but to the vertebræ of the fore part of the chest or of the hinder part of the neck.

February 28, 1859.

Dr. Donaldson read a paper "On Plato's Cosmical System as exhibited in the tenth book of 'The Republic.'"

The author first gave a translation of the whole passage (Plato, *Resp*. x. 616 B, 617 E), accompanied by a critical and philological examination of the Greek text. He then undertook to show the connexion between the fanciful picture of the universe which Plato has here given, with his other speculations on the origin of things, and especially with the occult philosophy of numbers. And he argued, finally, that the tradition preserved by Clement of Alexandria, which identifies Er, the son of Armenius, with Zoroaster, rests upon a foundation in fact; and that while there is good reason to believe that the doctrines of Heracleitus and Zoroaster agreed in many essential particulars, and that Plato was well acquainted with the speculations of the Ephesian philosopher, there are certain particulars in

the cosmical myth of 'The Republic' which agree exactly with the theories known to have been common to Zoroaster and Heracleitus.

March 14, 1859.

A paper was read "On the general principles of which the Composition or aggregation of Forces is a consequence." By Prof. De Morgan.

This paper examines the fundamental grounds of the composition or, as Mr. De Morgan calls it, *aggregation* of forces. By a *tendency* is meant anything which has both *magnitude* and *application*: by *application* is meant any notion which, not presenting the idea of magnitude, presents the idea of *opposition*. Two tendencies have a third tendency for their *aggregate*, to which they are jointly equivalent: and *equivalence* is any notion which, given that things equivalent to the same are equivalent to one another, satisfies the following postulates, which are the grounds of every method of aggregation known in mechanics.

1. Any two tendencies have one aggregate (0, the aggregate of counteraction being included among possible cases), and one only.

2. The magnitude of the aggregate, and its application relatively to the applications of the aggregants, depend only on the relative, and not on the absolute, applications of the aggregants.

3. The order in which tendencies are aggregated produces no effect either on the magnitude or application of the aggregate.

4. Tendencies of the same or opposite applications are aggregated by the law of algebraical additions.

From these postulates follow the following theorems:—

5. In any aggregate, the result of partial aggregation may take the place of its own aggregants.

6. Two tendencies cannot counteract one another unless they have equal magnitudes and opposite applications.

7. An aggregate has not more than one pair of aggregants, when the applications of the aggregants are given, and are different.

8. If the aggregants be altered in any ratio, without change of application, the aggregate is altered in the same ratio, also without change of application.

9. Any tendency may be disaggregated into two of any two different applications, neither of which is its own.

From the preceding it is proved,—

1. When by application is meant *direction*, the law of aggregation must be the well-known law of the aggregation of forces meeting at a point.

2. When by application is meant *choice of a point through which a given direction is to be drawn*, the law of aggregation must be the well-known law of aggregation of parallel forces.

In the case of translations and rotations, the postulates are all laws of thought; in the case of pressures, whether divergent or parallel, whether equilibrating or producing motion, all the postu-

lates contain results of experience. Accordingly, the multifarious proofs of the laws of aggregation, in the case of pressures, are not the mathematical playthings which they are often supposed to be from their grounds being insufficiently stated. If to the postulates necessary to make the law of aggregation a consequence, be added the following, "the velocity due to the aggregate of pressures in one given direction is the aggregate of the velocities due to the pressures taken separately," it follows, as a mathematical consequence, that the pressure varies as the velocity created by it in a given time. This great law of dynamics, therefore, is not fundamentally distinct from the laws of aggregation, but follows from them with the addition of a postulate more simple than itself.

A proof was also given by Professor Stokes, of the theorem that "Every Equation has a Root."

May 2, 1859.

Professor Miller made a communication "On the employment of the Gnomonic Projection of the Sphere in Crystallography."

May 16, 1859.

Mr. Hopkins gave a lecture "On Glacial Theories."

De Saussure attributed the motion of glaciers to their sliding over the bottoms of the valleys in which they exist, but did not appear to have made any observations on the change of form to which the glacial masses may be subject during their motion. No advance was made in our knowledge of glaciers for many years after De Saussure's death, till certain Swiss observers, Charpentier and others, rather more than twenty years ago, added much to our knowledge of the subject. Among these glacial observers, M. Agassiz, with characteristic zeal and activity, soon afterwards took a prominent position. He and those who were associated with him, or had preceded him, brought forward incontestible evidence of the former extension of glaciers in the Alps, and of their efficiency in the transport of enormous angular fragments of rock from their original sites to other localities, not only in the same Alpine valley, but even on the flanks of the Jura on the opposite side of the great central valley of Switzerland. These were the great facts which bore upon speculative geology. Glaciers were still engaged in the work of transport, and they had been so on a much larger scale at some former epoch; the masses of ice of which they were composed had been then of much larger dimensions, and consequently the mean climate of Western Europe must have been at that period considerably lower than at present. No one was so active as M. Agassiz in pressing these facts on the notice of geologists, or insisted more strongly on their geological importance; and by the energy of his own character, his great reputation, and extensive personal acquaintance with men

of science, he had undoubtedly made the first great steps in giving currency to the glacial theories of geology—theories which, though viewed at that time with much distrust, had since, with such modifications as enlarged knowledge and sober judgment had imposed on them, been universally recognized by geologists.

For his unflinching advocacy of the glacial theory in its broad outlines, geologists had been unquestionably much indebted to M. Agassiz; but in his physical theories respecting glacial phænomena, he had not shown that caution or acquaintance with physical science which the subject demanded. With respect to the motion of glaciers, it may be sufficient to state that he regarded it as due to the infiltration and subsequent freezing of water within the glacier, and a consequent expansion of its mass, by which the glacier in general, and especially those portions near its lower extremity, were urged forwards in the direction in which the bed of the glacier descended. Few persons ever received this theory, and it is no longer considered as deserving of serious attention.

A few years later Professor Forbes commenced his researches among the Alpine glaciers. His 'Travels through the Alps' was published in 1843, and contained a greater amount of well-arranged information respecting glacial phænomena than perhaps all other works together on that subject. But in this lecture, Mr. Hopkins remarked, he professed to deal with theories, and not with descriptive details. M. Agassiz's second work on glaciers, his *Système Glacière*, also appeared in 1847. Professor Forbes introduced a new view of the motion of glaciers, which he attributes to a certain facility with which he supposes glacial ice to be capable of changing its form under the pressures to which it is subjected, in a manner similar to that in which a *viscous* mass would change its form under the same circumstances. Hence it was called the *viscous theory*. It was founded on the fact (distinctly ascertained, Mr. Hopkins believed, the same year, both by Agassiz on the glacier of the Aar, and by Prof. Forbes on the Mer de Glace) that the central portions of a glacier moved considerably faster than its lateral portions, as a viscous mass would move along a trough inclined at a small angle to the horizon; and, moreover, it was obvious that the general mass of a glacier did so change its form as to accommodate itself to the changing dimensions of the valley down which it moved.

On the other hand, it was contended that a substance so hard and brittle as glacial ice could not be said to have the property of *viscosity*, and that the different velocities of the central and lateral portions of a glacier, and the changes of form which the general mass might undergo, were more attributable to the formation of crevasses and to discontinuous ruptures of the mass, than to any continuous change of form in each infinitesimal portion of it, like that which takes place in a mass which can be properly termed viscous. That this view was partly true was obvious, since ruptures and crevasses were actually formed by the unequal motions of different portions of the mass. Those who maintained this latter view held that the glacier moved by actually *sliding* over its bed; while those who sup-

ported the *viscous theory* contended that there was no such sliding motion, or if it existed at all, it constituted but a small part of the whole observed progressive motion of the surface of the glacier.

In the warmth of discussion these theories came to be considered as more antagonistic than they really were. It was manifestly possible that the lower surface of the glacier might *slide*, and thus cause a part of the observed motion of the upper surface; which might also have an additional motion, due to the more rapid progression of the upper portions of the mass as compared with that of its lower portions retarded by friction, as in the case of a semifluid mass. On this point Mr. Hopkins quoted the following passage from one of his letters "On the Mechanism of Glacial Motion," addressed to the editors of the Philosophical Magazine in 1844–45. If observations "should concur in showing an approximate equality in the motions of the upper and lower surfaces of a glacier, every candid and impartial mind must admit, I conceive, the *sliding* in preference to the *viscous* theory; but if, on the contrary, it should be proved that the velocity of the upper bears a large ratio to that of the lower surface, the claims of the latter theory must be at once admitted." Since this was written, several observations had been made by different persons, which agreed in showing that the upper surface of a glacier does move faster than the lower surface; but the only observations Mr. Hopkins had met with which enabled us to compare the actual amounts of those motions, had been made by Prof. Forbes himself near the extremity, Mr. Hopkins believed, of one of the glaciers at Chamouni. The result was that the upper surface moved about twice as fast as the lower one, thus proving that in this instance the motion of the upper surface was due in nearly equal degrees to the two causes above mentioned, and that both theories had so far equal claims to be admitted.

But at present no one probably doubted the fact of the whole motion of a glacier being made up of that motion which it derives from the property hitherto usually designated as the *viscosity* or *plasticity* of its mass, and that which consists of a *sliding* over its bed. Dr. Tyndall had recently observed proofs of this latter motion in various parts of glaciers as well as near their lower extremities; and all the phænomena of polished and striated rocks indicate most clearly that such motion must have existed in the ancient glaciers to which such phænomena are referred. But how was it conceivable that a glacier should thus *slide* over a surface on which there must be many and considerable inequalities, and at inclinations sometimes not exceeding 2° or 3°? And if it did thus slide, how was it that it did not move, as bodies ordinarily move down inclined planes, with an *accelerated* motion? These questions were frequently dwelt upon formerly. They were completely answered by the experiments made by Mr. Hopkins, and described by him in the 'Transactions' of this Society in 1844 (vol. viii. part 1), and in his first letter "On the Motion of Glaciers," dated November 19 of that year, and inserted in the Philosophical Magazine. The motion in question was not at all analogous to that of a body descending down

an inclined plane and retarded by friction as a constant force; it was due to the fact of the cohesion of the constituent particles of the mass at its lower surface being insufficient to resist the tendency of such an enormous weight of ice to descend down a plane even of very small inclination. A continuous disintegration is thus produced, promoted probably, in a greater or less degree, by a constant but very gradual thawing of the ice at the lower surface. In this manner it is easy to see that the motion must depend on the rate of disintegration, and therefore must be nearly a uniform, and not an accelerated motion.

Professor Forbes had an undoubted claim to the credit of being the first to suggest and insist upon the capability of the general mass of a glacier to change its form under existing conditions, as a cause of glacial motion. The above explanation of the sliding of the mass Mr. Hopkins claimed for himself.

Still it was felt that further investigation was required respecting the property of glacial ice which had been designated as its *viscosity*. There was no conclusive evidence that glacial ice would bear any considerable *extension* without breaking, for numberless crevasses were formed wherever the ice appeared to be subjected to any great extending force. Again, it was equally certain that the contiguous portions of a dislocated glacial mass, though retaining their perfect solidity, did become reunited into one continuous and unbroken mass. These facts were not sufficiently explained by the assertion that glacial ice was *viscous*. The true explanation appeared to have been afforded by an observation made some time ago by Dr. Faraday, and the more recent experiments of Dr. Tyndall. The former observed that two pieces of ice in perfect contact would freeze together so as to become one perfectly continuous mass, though the surrounding temperature should be much higher than 32°; and the latter gentleman had shown, by a striking form of the experiment, the extreme facility and rapidity with which a piece of common ice, after being crushed and broken into numberless fragments, will reunite into one continuous mass of transparent ice. This process had been designated by the term "*regelation*;" and manifestly some corresponding term was required to designate the property which, in ice, rendered that process so complete. Such terms as *viscous* and *plastic* failed to express adequately the property in question. At the same time it should be remarked that, so far as glacial motion depended on the facility with which the glacial mass might change its form, the manner in which that change was effected was of secondary importance, and did not diminish whatever value attached to Professor Forbes's first recognition of this change as an important cause of glacial motion. In one respect, however, the mechanism of the motion would be in some measure affected. Dr. Tyndall contends, and in a paper recently presented to the Royal Society has collected a considerable amount of evidence to show, that glacial ice would bear no more linear extension, independently of lateral compression, than ordinary specimens of ice would lead us to suppose, and consequently, when acted on by extending forces, it

cracked, forming fissures and crevasses to a much greater extent than would seem consistent with any property to which the term *viscous* could be applied with strict propriety.

Professor Forbes was also the first to make known to us, by systematic and well-directed observations, the facts and laws of the *veined structure* in glacial ice. He also entered into elaborate speculations on the causes which produced this structure, both in his 'Travels in the Alps,' and in letters written subsequently. Dr. Tyndall has also put forward a theory suggested by an analogy between the veined structure of ice and the lamination of rocks. As there appeared to have been some confusion as to the differences between these two theories, Mr. Hopkins would endeavour, as far as he was able, to explain them.

Both these theories depended primarily on the internal tensions and pressures to which the glacial mass might be subjected. The different parts of a glacier, as was well known, move with different velocities, the most general law being that the central move faster than the lateral portions; but whatever may lead to this unequable motion, its manifest result must be a tendency to *drag* the slower-moving portions of the ice after those which move more quickly. Moreover, it was easy to see that, in certain directions, this dragging might be greater on one portion of the ice than on a contiguous portion, and might thus tend to give different motions to contiguous *vertical slices* of the mass. This difference of motion, or *differential motion*, was supposed by Professor Forbes to actually exist, and that ruptures or breaches of continuity were absolutely produced between these vertical thin slices of ice by the strain upon them, and that these ruptures gave rise to the veined structure. His first idea appeared to have been that water infiltrated into the small fissures thus formed, where it afterwards froze and formed the veins of blue transparent ice, while the intermediate vertical laminæ contained a sufficient quantity of air-bubbles to render them white and opake. This notion of infiltration appeared to have been subsequently given up by the Professor, the conversion of the opake into transparent ice being supposed to take place by pressure and differential motion, independently, as far as Mr. Hopkins understood, either of infiltration or the melting of any portions of the ice.

Both Professor Forbes and Dr. Tyndall had endeavoured to elucidate the phænomena of glacial motion by means of a semifluid substance descending down a trough inclined to the horizon. For the purpose of ascertaining the direction of greatest extension and compression of the substance when thus put in motion, the latter gentleman described circles on its surface while still at rest, and observed the compressions and extensions of the radii when the mass was in motion. He thus found that the lines of greatest extension were inclined at angles of 45° to the axis of the trough; each such line pointing centrally and downwards, or laterally and upwards, while the lines of greatest compression were perpendicular to them. In the case of a glacier descending a canal-shaped valley, the former of these directions would manifestly be that of greatest tension;

and this is precisely the result which Mr. Hopkins had obtained both by exact mechanical reasoning and by experiment fifteen years ago; and it was thus that he was able to explain (and he was the first to do so) the formation of crevasses making angles of 45° with the axis of the glacier, and directed centrally and upwards, *i. e.* at right angles to the lines of greatest tension. There was also another result to which Mr. Hopkins was led by an exact consideration of the problem, but which he had also elucidated by experiments, as described in the letters above alluded to in the Philosophical Magazine. There were not only directions of maximum and minimum pressures and tensions at each point of the mass, there were also two other directions inclined at 45° to the former, not recognized by either of the above-mentioned experimenters, in which there is a maximum tendency to produce the *differential motion*, to which Prof. Forbes ascribed an actual rupturing of the ice and consequent formation of the veined structure. In the directions of maximum and minimum pressures or tensions, this tendency to produce a differential motion altogether vanished. The truth of these results was just as certain as that of the parallelogram of forces. In more complicated cases than that above supposed of a glacier descending down a canal-shaped valley, the absolute directions of these different lines would vary with the conditions of the glacial mass, and the external pressures to which it was subjected; but the important fact was, that there must in all cases exist at each point of the mass a direction of *maximum tension* or of *minimum pressure*, and a direction, perpendicular to it, of *minimum tension* or *maximum pressure*, and two other directions inclined to each of the former at 45°, along which there is a maximum tendency to produce the kind of differential motion above described. It was moreover manifest that where crevasses were formed there must be tension; and equally manifest that the directions of such crevasses must at least approximate to *perpendicularity with the directions of maximum tension*, and therefore to coincidence with those of maximum pressure. Also, if discontinuities and differential motions resulted from these internal pressures and tensions, they must be produced in those directions in which there is the maximum tendency to produce them, *i. e.* in directions inclined at 45° to those of the crevasses. All these directions might be supposed to be (as they would generally be in a glacier) nearly horizontal. These conclusions, Mr. Hopkins repeated, were as certain as that of the parallelogram of forces, and no theory which contradicted them could possibly be true.

Professor Forbes was the first to recognize the law which establishes a certain relation between the veined structure and the crevasses. He asserts that, as a matter of observation, the crevasses intersect the structure at right angles; consequently the blue veins must be perpendicular to the directions of maximum pressure, and could not coincide (such being the law) with the directions in which *differential motion* must necessarily take place, if it should take place at all. The law above enunciated exactly accorded with the conclusions above stated, and also with Dr. Tyndall's views, who asserts,

from his own observations, that the laminæ of blue and white ice (and especially in those places in which the structure originates) conform to the law of *perpendicularity to the direction of greatest pressure*. So far from there being any tendency to produce ruptures and fissures lying in the planes of the laminæ in these positions, they were the only positions entirely free from such tendency. And hence it became so difficult to conceive how the laminated structure could possibly originate in actual discontinuities such as those to which Professor Forbes had ascribed them, whether we suppose the blue laminæ to be produced by subsequent infiltration or any other process.

According to Dr. Tyndall's views, the law stated in the preceding paragraph was an essential consequence of physical causes, to which the production of the laminæ was referred. He had shown, experimentally, that if a piece of ordinary ice be subjected to direct pressure, it will melt along fine lenticular laminæ perpendicular to the direction of the pressure. A similar process is supposed to take place in the *névé*, or in any part of the glacier where the structural laminæ originate. Professor W. Thomson had offered an explanation of this phænomenon on thermal principles. It had been shown by himself and his brother that the melting temperature of ice is *lowered* by compression. Now if, in Dr. Tyndall's experiment, the ice were a perfectly homogeneous substance, every portion would be equally compressed; and if the uncompressed mass were only just above the melting temperature, the whole would melt under the compressing force. But no substance is perfectly homogeneous; and consequently the internal pressures in the experiment would not be perfectly equable; and those portions of the ice which were subjected to the greatest pressure would melt the soonest, and produce the aqueous laminæ above mentioned in the experiment, or the laminæ of blue ice in the glacier. In the latter case the laminar portions must first be supposed to melt, the air-bubbles to escape, and the water subsequently to be refrozen to form the blue transparent laminæ.

Mr. Hopkins did not profess to maintain the entire adequacy of the above explanation of the formation of the laminated structure, though he could not but feel persuaded that it was founded in truth. It seemed to explain very satisfactorily the conversion of *névé*, or opake white ice, into transparent blue ice; but he did not well understand how transparent consolidated ice could be converted by pressure into white opake ice. But still, at the bottom of an ice-fall, as that of the glacier of the Rhone, the broken fragments were again united into a continuous mass, and the laminated structure was reproduced on a type entirely new, and conformable to the altered conditions of the mass; and, assuming a large portion of the ice descending the fall to be in a sufficiently consolidated state, the process of reconstruction must consist as much in the conversion of blue ice into white, as of white ice into blue. Mr. Hopkins was not aware whether this difficulty had been previously started, or, if so, what answer had been made to it. His object was more especially to

point out the essential distinctions between Professor Forbes's and Dr. Tyndall's theories respecting the laminar structure. A disposition had recently manifested itself to confound the two theories, whereas they were so fundamentally different, that the physical reasoning essentially involved in the one was totally inapplicable to the other. The differences were such as could not be ignored, if we would hope to arrive at a complete and final view of the subject.

The remarks in this lecture, Mr. Hopkins said, had been made with a sincere desire of eliciting the truth, and not in the mere spirit of advocacy of preconceived opinions; nor would it, he conceived, be inconsistent with this assertion if, in conclusion, he reminded those who were interested in the subject, that though his own investigations nearly fifteen years ago respecting the internal pressures and tensions of glacial masses were little noticed then, and had been little mentioned more recently, no one had ever attempted to refute them; and now, on the contrary, all those observations and experiments of Dr. Tyndall which related to this part of the subject, and were at present generally received, were entirely confirmative of them. The nature of the reasoning which has now been applied to the subject, whether founded on analogies with certain phænomena of lamination, or on thermal principles, clearly proved the necessity of more accurate conceptions of these internal pressures and tensions than could ever be acquired from merely elucidatory experiments.

May 30, 1859.

"On the Occultation of Saturn by the Moon on May 8, 1859." By Professor Challis.

In observing this occultation, Professor Challis was prepared to take especial notice of the occurrence of any phænomenon like that witnessed at the occultation of Jupiter on January 2, 1857, on which occasion the disc of the planet at emergence was seen to be traversed by a dark band contiguous to the moon's limb. No such appearance was visible in this instance. The circumstances of the reappearance of Saturn at the moon's bright limb on May 8, were very similar to those of the reappearance of Jupiter, excepting that there was no depression of the limb where Saturn reappeared such as that which was noticed at the place of Jupiter's reappearance. The comparison of the two occultations seems, therefore, to indicate that the phænomenon seen in the case of Jupiter was in some way connected with the indented form of the moon's limb.

PROCEEDINGS

OF THE

CAMBRIDGE PHILOSOPHICAL SOCIETY.

October 31, 1859.

A communication was made by Mr. Hopkins "On the construction of a new Calorimeter for determining the Radiating Power of the Surfaces of Heated Bodies."

November 14, 1859.

A communication was made by the Master of Trinity College "On the Mathematical part of Plato's Meno."

November 28, 1859.

The Rev. Dr. Donaldson read a paper "On the Origin and proper value of the word 'Argument.'"

The author first investigated the etymology and meaning of the Latin verb *arguo*, and its participle *argutus*. He showed that *arguo* was a corruption of *argruo* = *ad gruo*; that *gruo* (in *argruo*, *ingruo*, *congruo*) ought to be compared with *κρούω*, which means "to dash one thing against another," especially for the purpose of making a shrill, ringing noise; that *arguo* means "to knock something for the purpose of making it ring, or testing its soundness," hence "to test, examine, and prove anything;" and that *argutus* signifies "made to ring," hence "making a distinct, shrill noise," or "tested and put to the proof." Accordingly *argumentum* means *id quod arguit*, "that which makes a substance ring, which sounds, examines, tests, and proves it."

It was then shown that these meanings were not only borne out by the classical usage of the word, but also by the technical application of "argument" as a logical term. For it is not equivalent to "argumentation," or the process of reasoning; it does not even denote a complete syllogism; though Dr. Whately and some other

writers on logic have fallen into this vague use of the word, and though it was so understood in the disputations of the Cambridge schools. The proper use of the word "argument" in logic is to denote "the middle term," *i. e.* "the term used for proof." In a sense similar to this the word is employed by mathematicians; and there can be no doubt that the oldest and best logicians confine the word to this, which is still its most common signification.

The author entered at some length into the Aristotelian definition of the *enthymeme*, which may be rendered approximately by the word "argument." He also explained how the words "topic" and "argument" came to denote the subject of a discourse or even of a picture. He showed, by a collection of examples from the best English poets, that the established meanings of the word "argument" are reducible to three: (1) a proof or means of proving; (2) a process of reasoning or controversy made up of such proofs; (3) the subject matter of any discourse, writing, or picture. And he maintained that the second of these meanings ought to be excluded from scientific language.

December 12, 1859.

The following paper from the Astronomer Royal was read:—"Supplement to the proof of the Theorem that 'Every Algebraic Equation has a Root.'"

The author expressed his want of confidence in every result obtained by the use of imaginary symbols, and in this supplement demonstrated that the left-hand member of every algebraic equation of the form $\phi(x)=0$ admitted of resolution, either into real linear factors, or into real quadratic factors.

Professor Miller also made a communication "On a new portable form of Heliotrope, and on the employment of Camera Lucida prisms and right-angled prisms in surveying."

February 13, 1860.

The Rev. H. A. J. Munro read a paper "On the Metre of an Inscription copied by Mr. Blakesley, and printed by him in his 'Four Months in Algeria,' p. 285."

February 27, 1860.

The Rev. Professor Sedgwick made the following communications:—

1. "An account of Mr. Barrett's progress in the Survey of Jamaica, with some remarks on the Distribution of Gold Veins."

2 "Some account of the Geological Discoveries in the Arctic Regions."

March 12, 1860.

The Rev. Professor Challis made a communication "On the Planet within the orbit of Mercury, discovered by M. Lescarbault."

By a recent comparison of the theory of Mercury's orbit with observation, M. Leverrier found that the calculated secular motion of the perihelion of that planet requires to be increased by 38″, and that this difference between observation and theory cannot be accounted for by the attractions of known bodies of the solar system. In a letter addressed to M. Faye, and published in the Paris Meteorological Bulletins of October 4, 5, and 6, 1859, he suggested that the difference might be due to the attraction of a group of small planets circulating between Mercury and the Sun. On December 22 of the same year, M. Lescarbault, a physician and amateur astronomer, residing at Orgères, about sixty miles south-west of Paris, announced in a letter to M. Leverrier that he had seen on March 26, 1859, a small round spot traversing the sun's disk, which he considered to be a planet inferior to Mercury. Naturally much interested by this information, M. Leverrier went to Orgères on December 31, and after closely interrogating M. Lescarbault respecting the particulars of the observation, and the instrumental means by which it was made, he returned with the conviction that the observation was trustworthy, and that a new planet had been discovered (*Comptes Rendus*, January 2, 1860, p. 40).

M. Lescarbault had long conceived the idea of detecting inferior planets by watching the sun's disk for transits, and in 1858 he put his project into execution. He was in possession of a good telescope of 3¾ inches aperture and 5 feet focal length, mounted with an altitude and azimuth movement, and provided with a finder magnifying 6 times. The power of the eyepiece employed in the observations of March 26 was 150. Not being furnished with a position-circle, he adopted the following means of obtaining angular measurements. The eyepiece of the telescope and the eyepiece of the finder each had at its focus two wires crossing at right angles, and the wires of the latter were so adjusted that a star seen at their intersection was seen at the same time at the intersection of the wires of the telescope. There were also in the eyepiece of the finder two wires parallel to, and on opposite sides of, each cross-wire, and distant by about 16′. A circular card about 6 inches in diameter, and graduated to half degrees, was placed concentric with the tube of the eyepiece of the finder, and apparently could be moved both about the tube and, with the tube, about the axis of the finder. A cross-wire of the telescope and a cross-wire of the finder were adjusted vertically by looking at a distant plumb-line, and the diameter of the card containing the zero of its graduation was placed vertically by means of a small plumb-line and eye-hole approximately arranged for that purpose. The

mode of using this apparatus for angular measurements will be seen by the following account of the observations. The observer had also a small transit-instrument by which he obtained true time, using for timepiece his watch, which, as it only indicated minutes, required the supplement of a temporary seconds' pendulum.

In the account which M. Lescarbault gives of his observations, he says that it had been his practice to examine with the telescope the contour of the sun for a considerable interval on each day in which he had leisure, and that at length, on March 26, 1859, he saw a small round spot near the limb, which he immediately brought to the intersection of the wires of the telescope. Then, according to his statement, he quickly turned the graduated card till *two* of the wires of the finder were tangents to the sun's limbs, or equidistant from them. But it is evident that to effect an angular measurement in this way, *one* of the middle wires of the finder must have been placed tangentially to the sun's limb at the point of their intersection, to which point the spot had just been brought. Assuming that this operation was performed, the angular distance of the point from the vertical diameter of the sun might be read off, as the account states that it was, by applying the plumb-line apparatus to the graduated card. This method could only give a rough measure of the angular position of a point very near the sun's limb; and in fact M. Lescarbault does not appear to have attempted to determine the position of the spot during the interval between the beginning and the end of the transit. He states that the spot had entered a little way on the sun when he first saw it, and that the time and place of entrance were inferred by estimation.

The following are the immediate results of the observations:— The spot entered at $4^h\ 5^m\ 36^s$ mean time of Orgères at the angular distance of 57° 22′ from the north point towards the west, and departed at $5^h\ 22^m\ 44^s$, at 85° 45′ from the south point towards the west, occupying consequently in its transit $1^h\ 17^m\ 8^s$. The length of the chord it described was 9′ 14″, and its least distance from the sun's centre 15′ 22″. M. Lescarbault also states that he judged the apparent diameter of the spot to be at most one-fourth of that of Mercury, when seen by him with the same telescope and magnifying power during its transit across the sun on May 8, 1845. The latitude of Orgères is 48° 8′ 55″, and longitude west of Paris, $2^m\ 35^s$.

From these data M. Leverrier ascertained, by calculating on the hypothesis of a circular orbit, that the longitude of the ascending node is 12° 59′, the inclination 12° 10′, the mean distance 0·1427, that of the earth being unity, and the periodic time 19·7 days. Also he found that the greatest elongation of the body from the sun is 8°, the inclination of its orbit to that of Mercury 7°, the real ratio of its diameter to Mercury's 1 to 2·58, and that its volume is one-seventeenth the volume of Mercury on the supposition of equal densities. This mass is much too small to account for the perturbation of Mercury's perihelion According to these results, the periods at which transits may be expected are eight days before and after April 2 and October 5, the body being between the earth and sun

near its descending node at the former period, and near its ascending node at the latter.

After the announcement of this singular discovery, it was found that other observations of a like kind had been previously made. Several instances are collected by Professor Wolf in the tenth number of his *Mittheilungen über die Sonnenflecken*, eight of which are quoted in vol. xx. (p. 100) of the Monthly Notices of the Royal Astronomical Society. Two of these, the observation of Stark on October 9, 1819, and that of Jenitsch on October 10, 1802, agree sufficiently well with the calculated position of the node of the object seen by Lescarbault. But the spot seen by Stark is stated to have been about the size of Mercury.

Capel Lofft saw at Ipswich, on January 6, 1818, at 11 A.M., a spot of a 'sub-elliptic form,' which advanced rapidly on the sun's disk, and was not visible in the evening of the same day (Monthly Magazine, 1818, part 1, p. 102).

Mr. Benjamin Scott, Chamberlain of London, saw about midsummer of 1847 a large and well-defined round spot, comparable in apparent size with Venus, which had departed at sunrise of the next day (Evening Mail, January 11, 1860).

Pastorff of Buchholz records that he saw on October 28 and November 1, 1836, and on February 17, 1837, *two* round black spots of unequal size, moving across the sun at the respective hourly rates of 14′, 7″, and 28′. Also he announced, January 9, 1835, to the Editor of the *Astronomische Nachrichten*, that "six times in the previous year he had seen two new bodies pass before the sun in different directions and with different velocities. The larger was about 3″ in diameter, and the smaller from 1″ to 1″·25. Both appeared perfectly round. Sometimes the smaller preceded, and at other times the larger. The greatest observed interval between them was 1′ 16″: at times they were very near each other. Their passage occupied a few hours. Both appeared as black as Mercury on the sun, and had a sharp round form, which, however, especially in the smaller, was difficult to distinguish." Schumacher considered it his duty as editor to insert the communication, but evidently did not give credit to it (*Astron. Nachr.* No. 273).

In vol. ii. of the Correspondence between Olbers and Bessel, mention is made in p. 162 of an observation at Vienna by Steinhübel, of a dark and well-defined spot of circular form which passed over the sun's diameter in five hours. Olbers, from these data, estimates the distance from the sun to be 0·19, and the periodic time thirty days. It is remarkable that Stark saw about noon *of the same day* a singular and well-defined circular spot, which was not visible in the evening. This is one of the instances in vol. xx. of the Monthly Notices of the Astronomical Society.

These accounts appear to prove that transits of dark round objects across the sun are real phænomena; but it would perhaps be premature to conclude that they are planetary bodies. If the object observed by Lescarbault be a planet, it is certainly very surprising that it has not been often seen. Schwabe, after observations of the sun's

face continued through thirty-three years, has recorded no instance of such a transit. It is probable that now attention has been especially drawn to the subject, future observations, accompanied by measures (of which Lescarbault's are the first instance), may throw light on the nature of these phænomena.

April 23, 1860.

Professor De Morgan read a paper "On the Syllogism, No. IV., and on the Logic of Relations."

In the third paper were presented the elements of a system in which only *onymatic* relations were considered; that is, relations which arise out of the mere notion of nomenclature—relations of name to name, as names. The present paper considers relation in general. It would hardly be possible to abstract the part of it which relates to relation itself, or to the author's controversy with the logicians, who declare all relations *material* except those which are onymatic, to which alone they give the name of *formal*. Mr. De Morgan denies that there is any purely formal proposition except "there is the probability *a* that X is in the relation L to Y;" and he maintains that the notion 'material' *non suscipit magis et minus*; so that the relating copula is as much materialized when for L we read *identical* as when for L we read *grandfather*.

Let X..LY signify that X stands in the relation L to Y; and X.LY that it does not. Let LM signify the relation compounded of L and M, so that X..LMY signifies that X is an L of an M of Y. In the doctrine of syllogism, it is necessary to take account of combinations involving a sign of *inherent quantity*, as follows:—

By X..LM′Y is signified that X is an L of *every* M of Y.

By X..L,MY it is signified that X is an L of *none but* Ms of Y.

The *contrary* relation of L, not-L, is signified by *l*. Thus X.LY is identical with X..*l*Y. The converse of L is signified by L^{-1}: thus X..LY is identical with Y..L^{-1}X. This is denominated the L-*verse* of X, and may be written LX by those who prefer to avoid the mathematical symbol.

The attachment of the sign of inherent quantity to the symbols of relation is the removal of a difficulty which, so long as it lasted, prevented any satisfactory treatment of the syllogism. There is nothing more in X..LM′Y than in every M of Y is an L^{-1} of X, or MY))L^{-1}X, X and Y being individuals; and nothing more in X..L,MY than in L^{-1}X))MY, except only the attachment of the idea of quantity to the combination of the relation.

When X is related to Y and Y to Z, a relation of X to Z follows: and the relation of X to Z is compounded of the relations of X to Y and Y to Z. And this is syllogism. Accordingly every syllogism has its inference really formed in the first figure, with both premises affirmative. For example, Y.LX and Y..MZ are premises stated

in the third figure: they amount to $X..L^{-1}Y$ and $Y..MZ$, giving $X..l^{-1}MZ$ for conclusion. This affirmative form of conclusion may be replaced by either of the negative forms $X.L^{-1}M'Z$ or $X..l_{,}{}^{-1}mZ$.

The arrangement of all the forms of syllogism, the discussion of points connected with the forms of conclusion, the extension from individual terms in relation to quantified propositions, the treatment of the particular cases in which relations are convertible, or transitive, or both—form the bulk of the paper, so far as it is not controversially directed against those who contend for the confinement of the syllogism to what Mr. De Morgan calls the *onymatic* form.

An appendix follows the paper, on syllogism of transposed quantity, in which the number of instances included in one premise is equal to the whole number of existing instances of the concluding term in the other premise.

Mr. J. H. Röhrs also read a paper "On the Motion of Bows, and thin Elastic Rods."

May 7, 1860.

The Rev. Professor Sedgwick made a communication "On the Succession of Organic Forms during long geological periods; and on certain Theories which profess to account for the origin of new species."

May 21, 1860.

The Public Orator read a paper "On the Pronunciation of the Ancient and Modern Greek Languages."

He gave a rapid sketch of the "Reuchlin and Erasmus" controversy in the sixteenth century, especially the part taken in it at Cambridge by Cheke, Smith, Ascham, and Bishop Gardiner; and then proceeded to show how the proper sounds of the Greek letters may be determined from the following sources:—

1. Distinct statements of grammarians.
2. Incidental notices in other ancient authors.
3. Variations in writing of inscriptions and MSS.
4. Phonetic spelling of cries of animals.
5. Puns and riddles.
6. The value of the respective letters in other languages employing the same alphabet, especially Latin.
7. The way in which Latin proper names are spelt in Greek, and *vice versâ*.
8. The traditions of pronunciation preserved in modern Greek.

He concluded that, on the whole, the method of Erasmus approached more nearly to the ancient pronunciation than that of Reuchlin.

"But," he proceeded, "when we consider the untrustworthiness of each of these sources of evidence taken singly, and when moreover we find them often in conflict with one another, it cannot be expected that the result should be very certain or very satisfactory. There are also other considerations which enhance the difficulty of the inquiry. As there were very marked dialectic varieties in Greece, so there may have been local variations even in Attica itself.

"The pronunciation, too, changed from time to time. Plato gives us proof of this in the 'Cratylus.'"

After quoting several instances, and showing that great changes both in pronunciation and spelling had taken place in modern languages, French, Spanish, and English, "it would," he said, "be hopeless to attempt to determine the pronunciation of any language by a reference to its orthography at a time when both were perpetually changing. But in the history of every nation there arrives a time when the creative energy of its literature seems to have spent itself; when, instead of developing new forms, men begin to look back and not forward, to comment and to criticise. Then it is that a language begins to assume, even in minor and merely outward points, such as pronunciation and spelling, a fixity and rigidity which it retains with scarcely any change so long as the nation holds together. Such a period in Greek history was that which began with the grammarian sophists in the fifth century B.C., and culminated in Aristarchus and Aristophanes of Byzantium. In the spelling and pronunciation of Greek there was probably very little change from that time to the end of the third century A.D."

October 19, 1860.

Dr. Paget made a communication "On some Points in the Physiology of Laughter."

November 12, 1860.

The Public Orator read a paper (a sequel to that on May 21) "On the Accentuation of Ancient Greek."

The question of accents was not discussed in the Reuchlin and Erasmus dispute. At that time all pronounced according to the system of accents introduced by the Greeks of Constantinople, who first taught the ancient language to the Italians.

It was probably in Elizabeth's reign that we began to disuse the old pronunciation of vowels both in Greek and Latin; and concurrently with this change we, as well as the other nations of Europe, began to pronounce Greek, not with the modern Greek, but with the Latin accent. The reasons were:—

1. Teachers speaking the modern Greek were no longer required, so the tradition was not kept up.

2. It saved much trouble to pronounce both languages with the same accentuation.

3. The Greek accent perpetually clashes with quantity; the Latin much more rarely; never, indeed, in that syllable of which the quantity is most marked—the penultima.

Isaac Vossius (1650–60) advocated the disuse of accentual marks altogether, as the invention of a barbarous age to perpetuate a barbarous pronunciation.

After showing the meaning of the word 'accent' as applied to modern languages, and discussing the accentuation of the German, English, French, &c., he proceeded to say:

"There are three methods of emphasizing a syllable:—

1. By raising the note;
2. By prolonging the sound;
3. By increasing its volume.

"Scaliger, *De Causis Linguæ Latinæ*, lib. ii. cap. 52, recognizes this division when he says that a syllable may be considered of three dimensions in sound, having height, length, and breadth.

"Now in our own language, when we accent a syllable, which of these dimensions do we increase? Generally all three, but not necessarily; for when the prayers, for example, are intoned, *i. e.* read upon one note, the accent is marked by increasing the volume of sound (the third method), which involves also a longer time in utterance, *i. e.* a lengthening of quantity. In speaking, all three methods are employed, but one more prominently than the other, according to individual peculiarities of the speakers. What we blend, the Greeks kept distinct.

"We cannot understand the Greek system unless we bear this in mind. They never confounded accent with quantity. Ineradicable habit prevents us from reverting in practice to their method, just as they would have been unable to comprehend ours.

"It is clear from Dionysius, *De Comp. Verb.* lib. xi. cap. 75, that the dialogue in tragedy preserved the ordinary accentuation, which was disregarded only in choral passages set to music."

The practical conclusion was this: that while it would be desirable, if possible, to return to the Erasmian system of pronunciation, it would be extremely absurd to adopt the barbarous accentuation of modern Greek, which has quite lost the old essential distinction between accent and quantity. In this respect, as we cannot recover practically the ancient method, it is better to keep to our own system of the Latin accent, which does not confuse the learner's notion of quantity in verse as the modern Greek does.

An Athenian boy has the greatest difficulty in comprehending the rhythm of Homer or Sophocles. Hence it is not blind prejudice (as Professor Blackie asserts) which makes us keep to our old usage, but a well-grounded conviction that we should lose more by changing than we should gain.

November 26, 1860.

Professor Challis made a communication "On the Solar Eclipse of July 18, 1860."

December 10, 1860.

Mr. Seeley read a "Notice of Opinions on the Red Limestone at Hunstanton."

Professor Miller also described "An Instrument for measuring the radii of arcs of Rainbows."

February 11, 1861.

Mr. H. D. Macleod read a paper "On the present State of the Science of Political Economy."

The writer took a general survey of the science as it at present exists, testing several generally received doctrines by the principles of inductive logic, and earnestly enforcing the necessity of a thorough reform of the whole science, which must be constructed on principles analogous to those of the other inductive sciences.

February 25, 1861.

Dr. Humphry made a communication "On the Growth of Bones."

March 11, 1861.

The Master of Trinity made a communication "On the Timæus of Plato."

PROCEEDINGS

OF THE

CAMBRIDGE PHILOSOPHICAL SOCIETY.

1843–65.

VOL. I PART 16

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PROCEEDINGS

OF THE

CAMBRIDGE PHILOSOPHICAL SOCIETY.

October 26, 1863.

A communication was made by Mr. H. D. Macleod "On the Theory of Banking."

November 9, 1863.

Communications were made by Dr. Humphry, "The Results of Experiments on the Growth of the Jaw."

By Mr. Todhunter "On a Question in the Theory of Probabilities."

November 23, 1863.

A communication was made by Professor Challis "On the Meteor of August 10, 1863."

December 7, 1863.

A communication was made by Dr. Akin "On the Origin of Electricity."

February 8, 1864.

A communication was made by Professor Liveing "On the new Metal, Thallium."

V. **That the fund be vested in the Cambridge Philosophical Society, and the prize adjudged by three Fellows of that Society, nominated by the Council of the Society for each occasion.**

VI. **That, in the event of any difficulty arising in carrying out the above provisions in any particular instance, either from lack of a prize-subject of sufficient merit, or from any other cause, the Council of the Cambridge Philosophical Society be at liberty to carry over the amount of the Prize for that term towards augmenting the fund for future prizes, or to award it to some one not a Member of the University.**

On Capillary Attraction. By Mr POTTER.

May 13, 1867.

The PRESIDENT (H. W. COOKSON, D.D., Master of *St Peter's College*) in the Chair.

The Treasurer made his financial statement, his accounts were passed, and the thanks of the Society were returned to him.

On Modern Musical Scales. By HARVEY GOODWIN, D.D., *Dean of Ely.*

THERE are two points connected with the system of musical scales universally adopted in Europe in modern times, upon which I have long desired to have clearer notions than I have been able to gain from books.

I. The first is the principle upon which we pass from one key to another by the introduction of a new sharp or flat into the signature.

II. The second is the reason why the division of the notes in the ordinary diatonic scale, artificial as it manifestly is, has become the universal division of European music, and appears so simple and natural.

Upon these two points I propose to offer some suggestions in this paper, premising to professional musicians (if the paper should fall under the eye of such) that musically I write only as an amateur. This, however, is perhaps of no great importance, as the question is one rather of numbers and mathematics than of technical musical knowledge.

I. It is well known to every one acquainted with music that the ordinary musical keys, having for their signatures respectively *no* sharps, *one* sharp, *two* sharps, &c. are formed each from the other by sharpening the subdominant of the scale, and so bringing it within half a tone of the dominant to which in the next scale it becomes the leading note. Thus we pass simply from any key to the key of the dominant, and the transition is so easy that even in the least complicated compositions, as for example in hymn-tunes, the modulation constantly takes place. The tonics or key-notes in the scales thus formed are C, G, D, A, E, B.

Now it is not very easy to see the manner in which these successive scales are related to each other, nor why the system is so complete as it appears to be. But the relation may be exhibited to the eye, and the symmetry of the system consequently made plain, in the following manner.

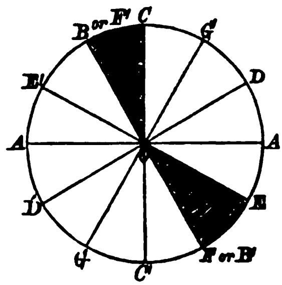
Let us regard the semitone intervals of the chromatic scale as being all equal, which, though not true upon any theoretical principle, is true according to that system of temperament, upon which pianofortes have long been tuned and upon which (as I understand upon good authority) it is now becoming the practice to tune organs. Then the musical interval between each note and its octave will be divided into twelve equal intervals, and these intervals may be conveniently represented

by angular spaces of 30° each, the whole twelve thus amounting to 360°, and so representing what may be regarded as the actual coincidence to the ear of the tonic and its octave.

The meaning of this will be seen from the annexed figure.

With centre O describe a circle, and divide the circumference into twelve equal parts. Join the dividing points with the centre, and put letters as in the figure.

Fig. I.



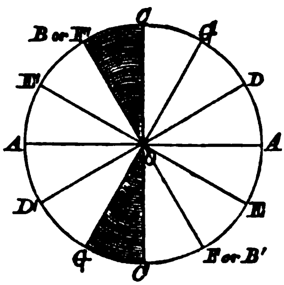
It will be observed that each diameter has the same letter at each extremity, the letters at the two extremities being distinguished by an accent affixed to one of them; it will also be observed that one diameter has an ambiguity, being either the diameter BOB', or the diameter FOF'; this arises from the fact that in the natural diatonic scale F is only a semitone removed from E, and B only a semitone from C. In the figure the angular spaces BOC, EOF have been shaded, to indicate at once to the eye that these are the two semitone intervals in the diatonic scale.

For the sake of distinctness, call COC' the *tonic line*, and GOG' the *dominant line*. Then it will be seen that the tonic line and the dominant line are inclined to each other at an angle of 30°: they are in fact next to each other in the group of note-lines which have been drawn through O.

It will be seen also that the shaded spaces corresponding to the two semitones are situated symmetrically with respect to DOD', the line corresponding to the second note of the scale.

Now let us see what effect will be produced upon our figure by sharpening the subdominant F. The arrangement will then

Fig. II.



be as in Fig. II. Comparing Fig. II. with Fig. I. it will be seen that the shaded spaces of Fig. II. are symmetrical with respect to AOA', as those of Fig. I. were with respect to DOD'; hence A is the second note of the new scale, in other words the key-note is G. For it will be observed that Fig. II. represents a scale of precisely the same kind as Fig. I.: in each case the shaded portions occupy one side of a diameter, leaving one semicircular space wholly unshaded, and containing between them an unshaded section of 120°. The eye will in fact at once perceive that if we start from G in Fig. II. and pass round the circumference of the circle in the sense of the motion of the hands of a clock, we shall come to exactly the same succession of tones and semitones as we should in Fig. I. if we started from C.

GOG', then, has now become the *tonic line;* but this line, regarded merely as to its direction, (I mean, GOG' being regarded as the same line as $G'OG$,) is removed only one division, or 30° degrees, from the original tonic line. Hence it may be said, that the effect of sharpening the subdominant is to turn the tonic line through one semitonal angular space, or through 30°; and as the sharpening of the subdominant of the key of C has brought us to the key of G, and turned the tonic line into the position GOG', so the sharpening of the subdominant of the key of G will turn the tonic line through another angle of 30° into the next position DOD', or will bring us to the key of D, and so on.

Hence if we drop the accents, the figures above drawn will give us, by looking at the extremities of the successive radii, the key-notes of the consecutive major scales, namely,

C, G, D, A, E, B.

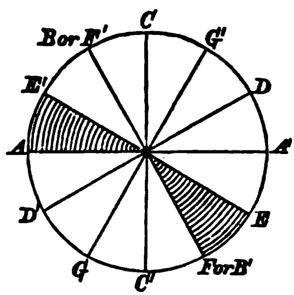
After this there is an apparent discontinuity, but not a real one, as the next key would be that of F♯, and this is in fact the key indicated by the line OC', being intermediate to OF and OG.

Thus the passage from one natural key to another is represented by the orderly revolution of a radius of our musical circle through angular spaces of 30°.

It is hardly necessary to say that the same method is applicable to the representation of the succession of flat keys: but it may be interesting to exhibit the method to the eye.

The successive flat keys are produced by flattening the leading note, or seventh of the scale. If we perform this process upon Fig. I. we have the annexed figure. Here the shaded semitonal spaces are symmetrical with respect to GOG' exactly as in Fig. I. they were symmetrical with respect to DOD', and in Fig. II. with respect to AOA'. Consequently G is the second note of the scale, or F is the tonic.

Fig. III.



Hence for the flat keys our tonic line revolves through one angular space of 30° in the sense contrary to the motion of the hands of a clock, or contrary to that in which it revolves for the sharp keys; and therefore Fig. III. will give us for the key-notes corresponding to the signatures one, two, three, four, five flats, the following

$$F,\ E',\ A',\ D',\ G',$$

or, as will be seen by inspecting the figures,

F, B♭, E♭, A♭, D♭.

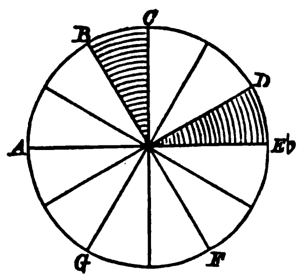
II. I now pass on to the second subject which I proposed to discuss in this paper; and I shall endeavour to exhibit by the machinery already introduced the great convenience of the

arrangement of the notes in the diatonic scale, and to suggest grounds for believing that no other division would be equally convenient.

Let us resume Fig. I. It will be seen that the musical circle representing the diatonic scale is divided into twelve equal portions, and that these twelve equal portions are divided into two groups, one consisting of four portions, or *two* whole tones, the other consisting of six portions, or *three* whole tones, by the two shaded portions corresponding to the semitones. Hence it is obvious that the shifting of the semitones so as to take one from the larger division of *three* tones and to add it to the smaller division of *two* tones, will leave a musical circle divided exactly as before; that is, there will still be two great divisions of *two* tones and *three* tones respectively, separated by semitones. The arrangement reproduces itself.

Not only is this the case, but it is easy to see that no other arrangement of the semitones would produce the same result. Suppose for instance we have the circle divided as in Fig. IV., that is, into two groups of *one* tone and *four* tones respectively, separated by the two semitones; in other words, regarding C as the tonic, suppose that we have a *flat third*. Then it is manifest that by no shifting of the semitones can this arrangement be made to reproduce itself. In fact the problem of making such a self-reproducing scale is merely that of dividing 5 into two parts, such that if unity be taken from one and added to the other the two parts shall be the same as before. It is manifest that the division into 2 and 3 is the only solution.

Fig. IV.



The division of the circle represented in Fig. IV. is somewhat interesting from the fact that it is the actual division

in an ascending minor scale. The arrangements of the semitones in the ascending and descending scales of a minor key are, as every one acquainted with the elements of music knows, different, and the signature is that which corresponds to the *descending* scale, the semitones being put into their proper places in the *ascending* scale by means of two accidentals. The arrangement of the semitones in the descending scale, if represented according to the method of this paper, is the ordinary diatonic arrangement: for example, Fig. I. would represent the descending scale of A minor, if we pass round the circle from A in the sense opposite to the motion of the hands of a clock; in other words the key of A minor has the same signature as that of C major; and as the signature is thus taken from the descending scale the modulations from one key to another in the minor scales follow the same rule as those in the major; but this would not be the case if the arrangement of semitones were that which, because it is more pleasing to the ear, we adopt in the ascending scale.

These considerations seem valuable with reference to the question, What is the reason why in modern Europe the common diatonic scale has gained such universal acceptance? It is a mistake to suppose that it is a scale founded upon any natural necessity; if so, it would be universal, which is not the case. But this seems to be the fact, namely, that there is such a perfection in the arrangement as ensures its adoption as soon as known, and guarantees its permanence to the exclusion of all others, except so far as a different arrangement may sometimes produce a feeling of pleasure by its novelty or its eccentricity. Nor is it perhaps difficult to deduce the diatonic division from simple principles, and to shew that it is not so arbitrary as at first sight it may seem.

The first principle must be the identity of a note with its octave. I speak of the *octave*, and by so doing appear to anticipate the existence of eight notes in the scale; but I do not

intend to make this anticipation. I only use the word octave to describe that note which is produced by vibrations of air twice as rapid as those which produce the fundamental note. Experience teaches us that the coincidence of two notes so related is acoustically perfect, so that they may be regarded as the same, and we may with propriety speak of the upper C or the lower C, applying the same letter C to express two notes, which, mechanically speaking, differ from each other, but which musically may be regarded as identical.

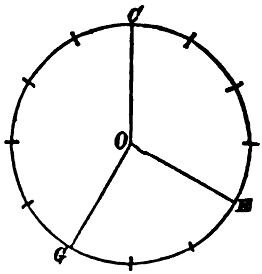
The question of the musical scale therefore resolves itself into that of interpolating a convenient number of conveniently related sounds between a note and its octave. Of all possible sounds which may be interpolated there are two which seem to have a chief claim to admission. These are the *third* and *fifth*, according to the common nomenclature; but mathematically speaking they are sounds produced by vibrations bearing a very simple numerical relation to those which produce the fundamental note, and musically speaking they are sounds which produce a very perfect harmony with the tonic and octave; and when the four notes are sounded successively, there is a simple and majestic progress from one to the other which every ear at once recognizes with pleasure.

So far all is tolerably simple, but the problem still remains to interpolate notes amongst those four, which we admit without question as the chief in the scale; and the problem branches out still further into the more complicated one of *temperament;* upon this many treatises have been written, and a compendious account of the question may be found in Sir John Herschel's Treatise on *Sound* in the *Encyclopædia Metropolitana*. It is not the purpose of this paper to enter into this difficult subject; but without doing so, I think it may be shewn by reference to the mode of illustration which has been adopted, that we are (as it were) forced into the arrangement of tones and semitones which constitutes the ordinary diatonic scale. For if we admit

the principle that a semitone is the smallest interval by which it is agreeable to the human ear that musical sounds should follow each other, and if we further admit that the third and fifth must find their place in the scale, then we find our musical circle divided into three portions which contain *about* (not accurately) 4, 3, and 5 semitones respectively.

No other division of these intervals seems possible, except that of dividing the first two intervals into two parts each, and the last into three. The positions of the two semitones, the existence of which is manifestly unavoidable, will still be undecided, but there will not be much difficulty in determining their position; for the satisfaction which the ear experiences in the sound of a *leading note*, or a note approaching within half a note of the tonic, is so great as to leave no doubt as to the position of the semitone between *G* and *C*; it must clearly be immediately contiguous to *C*; and this being so, it will be indifferent where we place the other semitones. For suppose we put it next to *E*, then we have the ordinary arrangement of the diatonic scale, the tonic being C: but suppose we take the other course and put it next to *G*, then the result is that the arrangement of semitones in the musical circle is exactly the same as before, only the tonic will be G instead of C. Hence, granting to the *third* and *fifth* their places in the scale *honoris causâ*, and allowing the necessity of a leading note, it appears that the arrangement of the semitones in the scale *must* be that with which we are familiar. And thus we seem to get at a *rationale* of the ordinary system of notes, which is in some respects more instructive than that which is usually given, as for example in the treatise of Sir John Herschel above cited; for the reasons there adduced depend upon considerations of the

Fig. V.



number of harmonies which can be made amongst the various notes of the octave or of successive octaves; and these considerations are valuable; but there would seem to be a propriety in the arrangement of the notes independent of them; there is a stately march of sound in the ordinary gamut which is highly satisfactory to the ear, and for which considerations of harmony do not seem to me to account.

One more point occurs to me as worthy of notice. I have spoken of the third and fifth of the scale as claiming their places before all other notes. There can be no doubt of this as regards the fifth; it fully deserves the title of the *dominant;* when we listen to a piece of music, its sound is left upon the ear almost, if not quite, as clearly as that of the tonic itself; and mathematically speaking the numbers which denote the ratio of its vibrations to those of the tonic are simpler than in the case of any other notes. But there may be a demur to the same precedence being granted to the third, at least to the *major* third, because it may be argued that the succession of notes is as pleasing and satisfying to the ear if for the major we substitute a minor third; that is, if we take as the basis of our system of notes the succession

C, E♭, G, C,

instead of

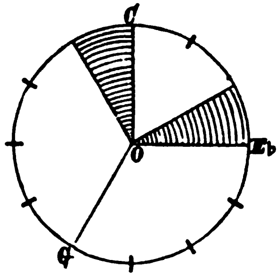
C, E, G, C.

Let us, then, just examine the conclusions to which we shall be led, if we start with the minor third as one of the primary intervals.

Our musical circle will now be as in Fig. VI.; and the question will be, where shall we put the semitones? In the ascending scale the demand for a leading note will lead us to put one of them below the upper C; and with regard to the other we easily perceive that the effect of the flat third is lost unless the note preceding it be distant by a small interval or by a semitone; not to mention that the juxtaposition of

two semitones, which is our only alternative, would be intolerable. Hence we arrive at the arrangement shewn in Fig. VI., which is that of the ordinary ascending minor scale; but it has been already pointed out that a scale of this kind does not admit of being changed into another, as does the diatonic arrangement; and it is fortunate that in the descending scale the ear does not by any means demand the small interval of the semitone between the eighth and seventh of the scale; on the other hand the peculiar and indescribable effect of the minor key, of which the flat third is the mainspring, is increased by dropping a whole tone instead of half from the eighth to the seventh. Taking, therefore, the descending scale and beginning with a fall of a whole note, the position of the other semitone settles itself; for, whether we put it third in the scale or fourth, it will be seen from inspection of the figure that we get no different arrangement of the notes: in each case we shall have two groups of two and three tones respectively separated by two semitones; in other words, in each case we shall have the ordinary diatonic arrangement.

Fig. VI.



In concluding this little essay, I will express the hope that the views which have been propounded, and the method employed for their illustration, may tend to give simplicity and clearness to a branch of science, the fundamental principles of which, though in some respects easy and familiar, are in others not free from obscurity.

H. GOODWIN.

May 27, 1867.

Professor CHALLIS (VICE-PRESIDENT) in the Chair.

The following were elected Honorary Members of the Society:

MM. EHRENBERG,
„ PONCELET,
„ PLÜCKER,
„ AGASSIZ,
„ QUETELET,
Dr DAUBENY,
Dr TODD,
Mr STEPHEN SMITH,
Mr MAX MÜLLER.

(1) *On a New Method of maintaining the Oscillations of a Pendulum. By* W. H. MILLER, M.A., FOR. SEC. R.S., *Professor of Mineralogy in the University of Cambridge.*

Professor Challis expressed his admiration of Professor Miller's contrivance, and described the practical difficulties which he had experienced with ordinary pendulums at the Observatory.

(2) *On the Crystallographic Method of Grassmann, and on its employment in the investigation of the general geometric properties of Crystals. By* PROFESSOR MILLER.

INTRODUCTION.

1. The law to which the mutual inclinations of the faces and cleavage planes of a crystal are subject, as enunciated by a large majority of the writers on Crystallography, is essentially embodied in the following statement:—

If through any point within a crystal planes be drawn parallel to each of its faces and cleavage planes, and any three of the straight lines in which these planes intersect one another, not being in one plane, be taken for axes, the equation to any face or cleavage plane of the crystal will be

$$h\frac{x}{a} + k\frac{y}{b} + l\frac{z}{c} = d,$$

where a, b, c are any three straight lines the ratios of which depend upon the species of the crystal, and the selection of axes, d is any positive quantity, and h, k, l are any positive or negative integers one or two of which may be zero.

A very different method was invented by Grassmann, who tells us that the difficulty of following the combinations of planes in the imagination, led him to the idea of substituting for the plane surfaces of crystals, normals to those surfaces or rays as he terms them. In other words, instead of the crystal he employs its reciprocal figure, adopting the definition of reciprocal figures given by Professor James Clerk Maxwell in the Philosophical Magazine for April, 1864. Grassmann was followed in the use of this method by Hessell in the Article *Krystall* in Gehler's Physikalisches Wörterbuch, reprinted separately under the title Krystallometrie, Leipzig, 1831; by Frankenheim in 1832, in a very elegant investigation of certain geometrical theorems, Einige Sätze aus der Geometrie der geraden Linie, Crelle, B. 8, S. 178; and lastly by Uhde, Versuch einer genetischen Entwickelung der mechanischen Krystallizations-Gesetze, Bremen, 1833. Later, however, this method appears to have been treated with a neglect it little deserves, for it possesses all the advantages of simplicity claimed for it by Grassmann, it leads directly to Neumann's representation of a crystal by the poles of its faces, and admits readily of the application of analytical geometry of three dimensions, ordinary geometry, or spherical trigonometry, in the investigation of the geometrical properties of crystalline forms. And though it may

not have led to any result that has not been obtained by the more usual method of treating the subject, an acquaintance with it can hardly fail to impart a clearer insight into the complicated relations of crystalline forms, and afford a fresh instance of the truth of the remark made by Sir John Herschel (Astronomy, 7th Edition, p. 6) that it is always of advantage to present any given body of knowledge to the mind in as great a variety of lights as possible.

2. According to Grassmann, if from any point within a crystal lines be drawn normal to the several faces of the crystal, and any three of these normals, not all in one plane, be taken for axes, the equations to any other normal will be

$$\frac{x}{h\alpha} = \frac{y}{k\beta} = \frac{z}{l\gamma},$$

where α, β, γ are three straight lines the ratios of which depend upon the species of the crystal and the selection of axes, and h, k, l are any integers either positive or negative or zero, one at least remaining finite. That these two enunciations lead to identical results, though not at first sight obvious, admits of an easy proof.

3. In fig. 1 let O be any point within a crystal. Let the surface of a sphere described round O as a centre meet the axes in X, Y, Z. Let ABC be the polar triangle of XYZ, and therefore OA, OB, OC radii normal to the faces $1\,0\,0$, $0\,1\,0$, $0\,0\,1$; P the pole of the face $h\,k\,l$; and a, b, c the parameters of the crystal.

Let L be the intersection of the great circles BC, AP. Through any point R in the straight line OP draw RQ parallel to OA, meeting the straight line OL in Q. Through Q draw QN parallel to OB meeting OC in N. Let $QR = x$, $NQ = y$, $ON = z$. It is proved in my Tract on Crystallography (4) that

$$\frac{k}{b}\sin BAP = \frac{l}{c}\sin CAP, \frac{l}{c}\sin CBP = \frac{h}{a}\sin ABP,$$

$$\frac{h}{a}\sin ACP = \frac{k}{b}\sin BCP.$$

But $\qquad \sin AB \sin BAP = \sin BL \sin BLP,$

and $\qquad \sin CA \sin CAP = \sin CL \sin CLP.$

Hence

$$\frac{y}{z} = \frac{\sin NOQ}{\sin OQN} = \frac{\sin CL}{\sin BL} = \frac{k}{b}\frac{c}{l}\frac{\sin CA}{\sin AB}.$$

In like manner $\qquad \dfrac{z}{x} = \dfrac{l}{c}\dfrac{a}{h}\dfrac{\sin AB}{\sin BC}.$

Also

$$\frac{\sin BC}{\sin A} = \frac{\sin CA}{\sin B} = \frac{\sin AB}{\sin C}, \quad \frac{\sin YZ}{\sin X} = \frac{\sin ZX}{\sin Y} = \frac{\sin XY}{\sin Z};$$

and, since ABC is the polar triangle of XYZ, $\sin YZ = \sin A$, $\sin ZX = \sin B$, $\sin XY = \sin C$. Therefore the equations to OR, a normal to the face $h\,k\,l$, will be

$$\frac{x}{h\alpha} = \frac{y}{k\beta} = \frac{z}{l\gamma},$$

where α, β, γ are three straight lines subject to one of the four indentical conditions

$$\frac{a\alpha}{\sin BC} = \frac{b\beta}{\sin CA} = \frac{c\gamma}{\sin AB}, \qquad \frac{a\alpha}{\sin A} = \frac{b\beta}{\sin B} = \frac{c\gamma}{\sin C},$$

$$\frac{a\alpha}{\sin YZ} = \frac{b\beta}{\sin ZX} = \frac{c\gamma}{\sin XY}, \qquad \frac{a\alpha}{\sin X} = \frac{b\beta}{\sin Y} = \frac{c\gamma}{\sin Z}.$$

It is evident that OR is the diagonal of a parallelopiped having its edges in the lines OA, OB, OC, and respectively proportional to $h\alpha$, $k\beta$, $l\gamma$.

Let G be the pole 1 1 1.

Then

$$\frac{1}{b}\sin BAG = \frac{1}{c}\sin CAG, \quad \frac{1}{c}\sin CBG = \frac{1}{a}\sin ABG,$$

$$\frac{1}{a}\sin ACG = \frac{1}{b}\sin BCG.$$

Therefore

$$k\frac{\sin BAP}{\sin BAG} = l\frac{\sin CAP}{\sin CAG}, \quad l\frac{\sin CBP}{\sin CBG} = h\frac{\sin ABP}{\sin ABG},$$

$$h\frac{\sin ACP}{\sin ACG}=k\frac{\sin BCP}{\sin BCG}.$$

This is equivalent to the form in which the law was enunciated by Gauss (C. F. Gauss, Werke, Band II. S. 308).

4. It remains to be seen whether the symbol of a zone has any geometrical signification when the normals to the faces are referred to OA, OB, OC as axes.

It appears from what precedes, interchanging a, b, c and α, β, γ, that

$$\frac{x}{ha}=\frac{y}{kb}=\frac{z}{lc}, \text{ and } h\frac{x}{\alpha}+k\frac{y}{\beta}+l\frac{z}{\gamma}=0,$$

being the equations to a line and plane, one referred to the axes OX, OY, OZ, and the other to the axes OA, OB, OC, the line will be normal to the plane. But (Tract 200), the equations to the zone-axis u v w referred to the axes OX, OY, OZ, are

$$\frac{x}{\mathrm{u}a}=\frac{y}{\mathrm{v}b}=\frac{z}{\mathrm{w}c}.$$

Hence, a plane through O, normal to the axis of the zone u v w, when referred to the axes OA, OB, OC, will have for its equation

$$\mathrm{u}\frac{x}{\alpha}+\mathrm{v}\frac{y}{\beta}+\mathrm{w}\frac{z}{\gamma}=0.$$

Let a plane parallel to the zone plane u v w meet OA, OB, OC in U, V, W. Then it is evident that

$$\mathrm{u}\frac{OU}{\alpha}=\mathrm{v}\frac{OV}{\beta}=\mathrm{w}\frac{OW}{\gamma}.$$

Let the axis of the zone u v w meet the surface of the sphere in K. It is easily seen that

$$\frac{\alpha}{\mathrm{u}}\cos AK=\frac{\beta}{\mathrm{v}}\cos BK=\frac{\gamma}{\mathrm{w}}\cos CK.$$

5. It appears then that the notation for faces and zones suggested by the equations to the faces and zone-axes, when the crystal is referred to three zone-axes as axes of coordinates, is equally applicable when the crystal is represented by rays normal to its faces, and these are referred to three such rays as

axes. Since the notation is the same in either case, it follows that the expressions for the geometric relations between faces and zones, in terms of their respective indices, will be absolutely the same whether we refer the faces to three zone-axes, or their rays to the three corresponding rays as axes of coordinates.

6. Instead of deducing the properties of a system of rays referred to three rays as axes, from those of faces referred to three zone-axes, it will be better to investigate the properties of Grassmann's system of rays without any reference to the crystal it is intended to represent. Having once established the properties of a purely geometric system of rays we may proceed to the consideration of crystals, avoiding the mistake sometimes made of anticipating the result of a geometrical investigation in the enunciation of a physical law, and assert that measurements of the angles between normals to the faces of crystals show that these normals are subject to the law according to which the system of rays was constructed, and that, consequently, all the geometrical properties of such a system of rays are properties of the rays drawn from any point within the crystal normal to its faces.

The analytical investigation is followed, first, by one in which ordinary geometry is used; and, afterwards, by an investigation of the properties of a system of points on the surface of a sphere, the points being the intersections of the rays with the surface of a sphere described round the origin of the system as a centre. Those propositions are omitted the investigation of which is the same whether we employ the polyhedral solid or its reciprocal figure.

Analytical Investigation of the Properties of a System of Rays.

Rays.

7. Let OA, OB, OC be any three straight lines not all in one plane; α, β, γ any three straight lines in a given proportion;

h, k, l any three integers positive or negative or zero, one of them at least remaining finite; let OA, OB, OC be taken as co-ordinate axes; and let a system of lines be constructed by giving different values to h, k, l in the equations

$$\frac{x}{h\alpha} = \frac{y}{k\beta} = \frac{z}{l\gamma}.$$

That portion of any line defined by the preceding equations, which lies on one side of the origin, will be called a ray. The ray containing the point $x = h\alpha$, $y = k\beta$, $z = l\gamma$ will be denoted by the symbol $h\,k\,l$. The ray containing the point $x = -h\alpha$, $y = -k\beta$, $z = -l\gamma$ will be denoted by the symbol $\bar{h}\,\bar{k}\,\bar{l}$. The integers $h\,k\,l$ will be called the indices of the ray; and the lines α, β, γ will be called the parameters of the system of rays thus constructed.

The lines OA, OB, OC are manifestly the rays 1 0 0, 0 1 0, 0 0 1.

Zone-planes.

8. The equations to the rays $h\,k\,l$, $p\,q\,r$ are

$$\frac{x}{h\alpha} = \frac{y}{k\beta} = \frac{z}{l\gamma}, \text{ and } \frac{x}{p\alpha} = \frac{y}{q\beta} = \frac{z}{r\gamma}.$$

Hence the equation to the plane containing the rays $h\,k\,l$, $p\,q\,r$ will be

$$\mathrm{u}\frac{x}{\alpha} + \mathrm{v}\frac{y}{\beta} + \mathrm{w}\frac{z}{\gamma} = 0,$$

where $\qquad \mathrm{u} = kr - lq, \ \mathrm{v} = lp - hr, \ \mathrm{w} = hq - kp.$

This plane will be called a zone-plane, and will be denoted by the symbol u v w. The quantities u, v, w will be called its indices. They are evidently positive or negative integers one or two of which may be zero.

The symbols of the zone-planes BOC, COA, AOB are 1 0 0, 0 1 0, 0 0 1 respectively.

The intersection of any two zone-planes is a ray.

9. The zone-planes h k l, p q r have for their equations

$$h\frac{x}{\alpha}+k\frac{y}{\beta}+l\frac{z}{\gamma}=0, \text{ and } p\frac{x}{\alpha}+q\frac{y}{\beta}+r\frac{z}{\gamma}=0.$$

The equations to the intersections of these planes will be

$$\frac{x}{u\alpha}=\frac{y}{v\beta}=\frac{z}{w\gamma},$$

where $$u=kr-lq,\ v=lp-hr,\ w=hq-kp.$$

The quantities u, v, w are obviously integers, and therefore the intersection of any two zone-planes is a ray.

Condition that a ray may lie in a zone-plane.

10. Let the zone-plane p q r contain the ray $u\,v\,w$. The equations to the zone-plane and ray are

$$p\frac{x}{\alpha}+q\frac{y}{\beta}+r\frac{z}{\gamma}=0, \text{ and } \frac{x}{u\alpha}=\frac{y}{v\beta}=\frac{z}{w\gamma}.$$

Therefore, since the plane contains the ray,

$$pu+qv+rw=0.$$

Portions of two rays cut off by parallels to two zone-planes.

11. In fig. 2 let OQ, OS be the rays $h\,k\,l$, $u\,v\,w$, and let the zone-plane QOS intersect the zone-planes e f g, p q r, in OP, OR. Let the planes having for their equations

$$e\frac{x}{\alpha}+f\frac{y}{\beta}+g\frac{z}{\gamma}=m,\quad p\frac{x}{\alpha}+q\frac{y}{\beta}+r\frac{z}{\gamma}=n,$$

and therefore parallel to the zone-planes e f g, p q r, meet the ray $h\,k\,l$ in D, Q, and the ray $u\,v\,w$ in F, S. Let planes passing through D, Q, F, S, parallel to the zone-planes 1 0 0, meet the ray 1 0 0 in d, q, f, s. Then Od, Oq, Of, Os will be the values of x at the points D, Q, F, S. Therefore, since the equations to the rays $h\,k\,l$, $u\,v\,w$ are

$$\frac{x}{h\alpha}=\frac{y}{k\beta}=\frac{z}{l\gamma}, \text{ and } \frac{x}{u\alpha}=\frac{y}{v\beta}=\frac{z}{w\gamma},$$

we shall have

$$(\mathrm{e}h + \mathrm{f}k + \mathrm{g}l)\,Od = mh\mathrm{z}, \quad (\mathrm{p}h + \mathrm{q}k + \mathrm{r}l)\,Oq = nh\mathrm{z},$$

$$(\mathrm{e}u + \mathrm{f}v + \mathrm{g}w)\,Of = mua, \quad (\mathrm{p}u + \mathrm{q}v + \mathrm{r}w)\,Os = nua.$$

But $Od : Oq = OD : OQ$, and $Of : Os = OF : OS$. Therefore

$$\frac{\mathrm{e}u + \mathrm{f}v + \mathrm{g}w}{\mathrm{e}h + \mathrm{f}k + \mathrm{g}l}\,\frac{OF}{OD} = \frac{\mathrm{p}u + \mathrm{q}v + \mathrm{r}w}{\mathrm{p}h + \mathrm{q}k + \mathrm{r}l}\,\frac{OS}{OQ}.$$

When only one of the rays OP, OR lies between OQ and OS, three of the points d, q, f, s will be on one side of O, and the fourth on the other side. Therefore $Of \cdot Os : Oq \cdot Od$ will be negative. When OP, OR are both without the angle OOS, or both within it, the points d, q, f, s will either be all on one side of O, or two on one side and two on the other side, and $Of \cdot Os : Oq \cdot Od$ will be positive. Hence the expression

$$\frac{\mathrm{e}h + \mathrm{f}k + \mathrm{g}l}{\mathrm{e}u + \mathrm{f}v + \mathrm{g}w}\,\frac{\mathrm{p}u + \mathrm{q}v + \mathrm{r}w}{\mathrm{p}h + \mathrm{q}k + \mathrm{r}l}$$

will be positive except when one only of the rays OP, OR lies between OQ and OS.

Anharmonic ratio of four rays in one zone-plane.

12. Since DF, QS are parallel to OP, OR respectively,

$$\sin POQ : \sin POS = \sin D : \sin F = OF : OD,$$

and $$\sin ROQ : \sin ROS = \sin Q : \sin S = OS : OQ.$$

Hence $$\frac{\sin POQ}{\sin POS}\,\frac{\sin ROS}{\sin ROQ} = \frac{\mathrm{e}h + \mathrm{f}k + \mathrm{g}l}{\mathrm{e}u + \mathrm{f}v + \mathrm{g}w}\,\frac{\mathrm{p}u + \mathrm{q}v + \mathrm{r}w}{\mathrm{p}h + \mathrm{q}k + \mathrm{r}l},$$

where OP, OQ, OR, OS are four rays in one zone-plane; e f g, p q r the symbols of zone-planes containing the rays OP, OR; and $h\,k\,l$, $u\,v\,w$ the symbols of the rays OQ, OS.

Anharmonic ratio of four zone-planes intersecting one another in one ray.

13. Retaining the notation of (12), let the zone-planes e f g, p q r intersect in the ray OK; and let a plane through O, in fig. 3,

normal to OK, meet the zone-planes e f g, p q r in Op, Or; KOQ, KOS in Oq, Os; and planes through DF, QS parallel to OK, in df, qs. Then df, qs will be parallel to Op, Or;

$$Od : Oq = OD : OQ\text{; and } Of : Os = OF : OS.$$

Therefore $\quad \sin pOq : \sin pOs = \sin d : \sin f = Of : Od,$

and $\quad \sin rOq : \sin rOs = \sin q : \sin s = Os : Oq.$

$$\text{Hence } \frac{\sin pOq}{\sin pOs}\frac{\sin rOs}{\sin rOq} = \frac{\mathrm{e}h + \mathrm{f}k + \mathrm{g}l}{\mathrm{e}u + \mathrm{f}v + \mathrm{g}w}\frac{\mathrm{p}u + \mathrm{q}v + \mathrm{r}w}{\mathrm{p}h + \mathrm{q}k + \mathrm{r}l},$$

where KOP, KOQ, KOR, KOS are four zone-planes intersecting one another in one ray; e f g, p q r the symbols of KOP, KOR; $h\ k\ l$, $u\ v\ w$ the symbols of rays contained in the zone-planes KOQ, KOS; pOq, pOs the angles which KOP makes with KOQ, KOS; and rOq, rOs the angles which KOR makes with KOQ, KOS.

Since the order of the zone-planes KOP, KOQ, KOR, KOS is the same as that of the rays OP, OQ, OR, OS, it follows from (11) that the expression which forms the right-hand side of the preceding equation is positive except when one only of the zone-planes KOP, KOR lies between the other two.

Indices of a ray when the axes are changed.

14. Let planes parallel to the zone-planes e f g, h k l, p q r meet the ray $m\ n\ o$ in D, L, Q, and the ray $u\ v\ w$ in F, N, S. Then (11)

$$\frac{\mathrm{e}u + \mathrm{f}v + \mathrm{g}w}{\mathrm{e}m + \mathrm{f}n + \mathrm{g}o}\frac{OF}{OD} = \frac{\mathrm{h}u + \mathrm{k}v + \mathrm{l}w}{\mathrm{h}m + \mathrm{k}n + \mathrm{l}o}\frac{ON}{OL} = \frac{\mathrm{p}u + \mathrm{q}v + \mathrm{r}w}{\mathrm{p}m + \mathrm{q}n + \mathrm{r}o}\frac{OS}{OQ}.$$

Let the zone-planes h k l, p q r intersect in the ray OA'; the zone-planes p q r, e f g in the ray OB'; and the zone-planes e f g, h k l in the ray OC'. And let $m'\ n'\ o'$, $u'\ v'\ w'$ be the symbols of the rays OQ, OS when referred to the rays OA', OB', OC' as axes. The symbols of the zone-planes e f g, h k l,

p q r when referred to the new axes will be 1 0 0, 0 1 0, 0 0 1 respectively. Therefore (11)

$$\frac{u'}{m'}\frac{OF}{OD}=\frac{v'}{n'}\frac{ON}{OL}=\frac{w'}{o'}\frac{OS}{OQ}.$$

Hence, comparing corresponding terms, two equations are obtained which are satisfied by making

$$m'=\mathrm{e}m+\mathrm{f}n+\mathrm{g}o,\quad u'=\mathrm{e}u+\mathrm{f}v+\mathrm{g}w,$$
$$n'=\mathrm{h}m+\mathrm{k}n+\mathrm{l}o,\quad v'=\mathrm{h}u+\mathrm{k}v+\mathrm{l}w,$$
$$o'=\mathrm{p}m+\mathrm{q}n+\mathrm{r}o,\quad w'=\mathrm{p}u+\mathrm{q}v+\mathrm{r}w.$$

The coefficients of u, v, w are integers, therefore u', v', w', the indices of the ray OS when referred to the rays OA', OB', OC' as axes, are also integers. Hence, the rays of the system are subject to the same law when referred to any three rays as axes, as when referred to the original axes.

Indices of a zone-plane when the axes are changed.

15. Let the rays $e\,f\,g$, $h\,k\,l$, $p\,q\,r$ meet a plane parallel to the zone-plane m n o in D, E, F, and a plane parallel to the zone-plane u v w in R, S, T. Then (11)

$$\frac{\mathrm{u}e+\mathrm{v}f+\mathrm{w}g}{\mathrm{m}e+\mathrm{n}f+\mathrm{o}g}\frac{OR}{OD}=\frac{\mathrm{u}h+\mathrm{v}k+\mathrm{w}l}{\mathrm{m}h+\mathrm{n}k+\mathrm{o}l}\frac{OS}{OE}=\frac{\mathrm{u}p+\mathrm{v}q+\mathrm{w}r}{\mathrm{m}h+\mathrm{n}q+\mathrm{o}r}\frac{OT}{OF}.$$

Let m′ n′ o′, u′ v′ w′ be the symbols of the zone-planes parallel to DEF, RST, when referred to the rays $e\,f\,g$, $h\,k\,l$, $p\,q\,r$ as axes. The new symbols of the rays OR, OS, OT will be 1 0 0, 0 1 0, 0 0 1. Therefore (11)

$$\frac{\mathrm{u}'}{\mathrm{m}'}\frac{OR}{OD}=\frac{\mathrm{v}'}{\mathrm{n}'}\frac{OS}{OE}=\frac{\mathrm{w}'}{\mathrm{o}'}\frac{OT}{OF}.$$

Hence, comparing corresponding terms, two equations are obtained which are satisfied by making

$$\mathrm{m}'=e\mathrm{m}+f\mathrm{n}+g\mathrm{o},\quad \mathrm{u}'=e\mathrm{u}+f\mathrm{v}+g\mathrm{w},$$
$$\mathrm{n}'=h\mathrm{m}+k\mathrm{n}+l\mathrm{o},\quad \mathrm{v}'=h\mathrm{u}+k\mathrm{v}+l\mathrm{w},$$
$$\mathrm{o}'=p\mathrm{m}+q\mathrm{n}+r\mathrm{o},\quad \mathrm{w}'=p\mathrm{u}+q\mathrm{v}+r\mathrm{w}.$$

Geometrical Investigation of the Properties of a System of Rays.

Rays.

16. In fig. 4 let O be the origin of a system of rays; OA, OB, OC the rays $1\,0\,0$, $0\,1\,0$, $0\,0\,1$; OR the ray $h\,k\,l$; HKL a parallelopiped having its edges in OA, OB, OC, and having OR for a diagonal. Then (7), since OH, OK, OL are the values of x, y, z for the point R, we shall have

$$\frac{OH}{h\alpha}=\frac{OK}{k\beta}=\frac{OL}{l\gamma}.$$

Zone-planes.

17. In OA, fig. 5, take $OU=-a$, and therefore measured from O in the direction opposite to A. Through U draw UM, US parallel to the rays $h\,k\,l$, $p\,q\,r$ respectively, meeting the plane BOC in M, S. Let MS meet OB in V, and OC in W. Draw MD, SG parallel to OC meeting OV in D, G. The lines UM, US are parallel to the rays $h\,k\,l$, $p\,q\,r$, therefore, observing that since $OU=-a$, $UO=a$,

$$\frac{UO}{h\alpha}=\frac{OD}{k\beta}=\frac{DM}{l\gamma}, \text{ and } \frac{UO}{p\alpha}=\frac{OG}{q\beta}=\frac{GS}{r\gamma}.$$

Hence

$$OD=\frac{k}{h}\beta,\ DM=\frac{l}{h}\gamma,\quad OG=\frac{q}{p}\beta,\quad GS=\frac{r}{p}\gamma.$$

The lines DM, GS are parallel to OW, therefore

$$OW:OV=DM:DV=DM-GS:OG-OD;$$

consequently

$$(hq-kp)\beta.OW=(lp-hr)\gamma.OV,\quad h(lp-hr).DV=l(hq-kp)\beta,$$

$$(lp-hr).OV=(lq-kr)\beta,\quad (hq-kp).OW=(lq-kr)\gamma.$$

Hence, if a plane parallel to the rays $h\,k\,l$, $p\,q\,r$ meet OA, OB, OC in UVW,

$$\frac{u}{\alpha} OU = \frac{v}{\beta} OV = \frac{w}{\gamma} OW,$$

where $\quad u = kr - lq, \quad v = lp - hr, \quad w = hq - kp.$

A plane through the rays $h\,k\,l$, $p\,q\,r$, and therefore parallel to the plane UVW, will be called a zone-plane, and will be denoted by the symbol u v w, or by any three integers respectively proportional to u, v, w; and the integers u, v, w, or any three proportional integers, will be called the indices of the zone-plane.

The intersection of any two zone-planes is a ray.

18. In OB, fig. 6, take $OB = \beta$. Let planes through B parallel to the zone-planes h k l, p q r intersect one another in the line BM which meets the plane COA in M; and let them meet OC in L, R, and OA in H, P. Then

$$\frac{h}{\alpha} OH = \frac{k}{\beta} OB = \frac{l}{\gamma} OL, \text{ and } \frac{p}{\alpha} OP = \frac{q}{\beta} OB = \frac{r}{\gamma} OR.$$

Therefore

$$l\,.\,OL = k\gamma, \quad h\,.\,OH = k\alpha, \quad r\,.\,OR = q\gamma, \quad p\,.\,OP = q\alpha.$$

Hence

$$lr\,.\,LR = (kr - lq)\gamma, \quad hp\,.\,HP = (hq - kp)\alpha.$$

But (Tract 187)

$$HM\,.\,OP\,.\,LR = HP\,.\,OR\,.\,LM.$$

Therefore, putting

$$u = kr - lq, \quad v = lp - hr, \quad w = hq - kp,$$

we have

$$wl\,.\,LM = uh\,.\,HM, \quad wl\,.\,LH = -vk\,.\,HM, \quad uh\,.\,LH = -vk\,.\,LM.$$

Draw MD parallel to OC meeting OA in D. By similar triangles $OD : LM = OH : LH$, and $DM : HM = OL : LH$. Hence $-v\,.\,OD = u\alpha$, and $-v\,.\,DM = w\gamma$. Draw MF equal and parallel to OB on the opposite side of the plane LOH. Then $-v\,.\,MF = v\,.\,OB = v\beta$. The line OF being parallel to BM, is

the intersection of the zone-planes h k l, p q r, and is evidently the diagonal of a parallelopiped the edges of which are respectively coincident with the axes of the system of rays, and equal to OD, MF, DM, and therefore proportional to

$$-v\,.\,OD,\quad -v\,.\,MF,\quad -v\,.\,DM,\quad \text{or to } u\alpha,\ v\beta,\ w\gamma.$$

Since u, v, w are integers, the line OF, in which the zone-planes h k l, p q r intersect, is a ray of the system, having $u\ v\ w$ for its symbol, where

$$u = \mathrm{kr} - \mathrm{lq},\quad v = \mathrm{lp} - \mathrm{hr},\quad w = \mathrm{hq} - \mathrm{kp}.$$

Portions of two rays cut off by parallels to two zone-planes.

19. Let a plane parallel to the zone-plane p q r meet the axes in I, J, K, fig. 7, and the ray $u\,v\,w$ in S. Draw KS meeting IJ in N, IS meeting JK in L, and ST parallel to OI, meeting the plane JOK in T. The symbols of the rays OK, OI, OS are 0 0 1, 1 0 0, $u\,v\,w$ respectively. Therefore the symbol of the zone-plane KOS will be $v\,\bar{u}\,0$, and that of the zone-plane IOS will be $0\,\bar{w}\,v$. The plane IJK is parallel to the zone-plane p q r. Hence the line KN will be parallel to the ray

$$-\mathrm{r}u,\quad -\mathrm{r}v,\quad \mathrm{p}u+\mathrm{q}v,$$

and the line IL will be parallel to the ray

$$\mathrm{q}v+\mathrm{r}w,\quad -\mathrm{p}v,\quad -\mathrm{p}w.$$

The lines KN, IL are in the plane IJK, therefore (18)

$$\mathrm{p}u\,.\,IN = \mathrm{q}v\,.\,JN,\ \text{and}\ \mathrm{q}v\,.\,JK = (\mathrm{q}v+\mathrm{r}w)\,.\,KL.$$

But (Tract 187) $\quad IS\,.\,KL\,.\,JN = SL\,.\,JK\,.\,IN.$

Hence $\quad \mathrm{p}u\,.\,IS = (\mathrm{q}v+\mathrm{r}w)\,.\,SL,$

and therefore $\quad \mathrm{p}u\,.\,IL = (\mathrm{p}u+\mathrm{q}v+\mathrm{r}w)\,SL.$

But $\quad ST : OI = SL : IL.$

Therefore $\quad \mathrm{p}u\,.\,OI = (\mathrm{p}u+\mathrm{q}v+\mathrm{r}w)\,.\,ST.$

In like manner, if a plane parallel to the zone-plane e f g meet

OI in E, and OS in F, and FG be drawn parallel to OI meeting the plane JOK in G, we shall have

$$\mathrm{e}u \,.\, OE = (\mathrm{e}u + \mathrm{f}v + \mathrm{g}w) \,.\, FG.$$

But $$OS : OF = ST : FG.$$

Therefore

$$(\mathrm{e}u + \mathrm{f}v + \mathrm{g}w) \,.\, OF : (\mathrm{p}u + \mathrm{q}v + \mathrm{r}w) \,.\, OS = \mathrm{e} \,.\, OE : \mathrm{p} \,.\, OI.$$

Hence if the ray $h\ k\ l$ meet planes parallel to the zone-planes e f g, p q r in the points D, Q, we shall have

$$(\mathrm{e}h + \mathrm{f}k + \mathrm{g}l) \,.\, OD : (\mathrm{p}h + \mathrm{q}k + \mathrm{r}l) \,.\, OQ = \mathrm{e} \,.\, OE : \mathrm{p} \,.\, OI.$$

Therefore

$$\frac{\mathrm{e}u + \mathrm{f}v + \mathrm{g}w}{\mathrm{e}h + \mathrm{f}k + \mathrm{g}l}\,\frac{OF}{OD} = \frac{\mathrm{p}u + \mathrm{q}v + \mathrm{r}w}{\mathrm{p}h + \mathrm{q}k + \mathrm{r}l}\,\frac{OS}{OQ}.$$

From this equation the anharmonic ratio of four rays in one zone-plane, of four zone-planes intersecting one another in one ray, and the indices of rays and zone-planes when the axes are changed, may be found as in (12), (13), (14), (15).

Properties of a System of Points on the Surface of a Sphere.

Poles.

20. Let the surface of a sphere having its centre in the origin of the system of rays meet the rays 1 0 0, 0 1 0, 0 0 1, 1 1 1 in A, B, C, G, and the ray $h\ k\ l$ in P, fig. 1. Let the great circle AP meet the great circle BC in L. From any point R in OP draw RQ parallel to OA meeting OL in Q. Draw QN parallel to OB meeting OC in N. Then (7), since QR, NQ, ON are the values of x, y, z at R,

$$\frac{QR}{h\alpha} = \frac{NQ}{k\beta} = \frac{ON}{l\gamma}.$$

But

$$ON : NQ = \sin BL : \sin CL = \sin AB \sin BAP : \sin CA \sin CAP.$$

Therefore

$$k\beta \sin AB \sin BAP = l\gamma \sin CA \sin CAP.$$

In like manner

$$l\gamma \sin BC \sin CBP = h\alpha \sin AB \sin ABP,$$

and $$h\alpha \sin CA \sin ACP = k\beta \sin BC \sin BCP.$$

Hence, $$\beta \sin AB \sin BAG = \gamma \sin CA \sin CAG,$$

$$\gamma \sin BC \sin CBG = \alpha \sin AB \sin ABG,$$

$$\alpha \sin CA \sin ACG = \beta \sin BC \sin BCG.$$

Therefore

$$k\frac{\sin BAP}{\sin BAG} = l\frac{\sin CAP}{\sin CAG},$$

$$l\frac{\sin CBP}{\sin CBG} = h\frac{\sin ABP}{\sin ABG},$$

$$h\frac{\sin ACP}{\sin ACG} = k\frac{\sin BCP}{\sin BCG}.$$

The point P will be called a pole, and will be denoted by the symbol of the ray intersected in that point by the surface of the sphere. The points A, B, C, G are the poles 1 0 0, 0 1 0, 0 0 1, 1 1 1 respectively.

Zone-circles.

21. In fig. 8 let A, B, C, G, P be the poles 1 0 0, 0 1 0, 0 0 1, 1 1 1, $u\,v\,w$ respectively. Let a great circle passing through P meet the great circles BC, CA, AB in D, E, F respectively. Then (Townsend's *Modern Geometry*, 82, Cor. 3), having regard to the signs of the six arcs,

$$\sin DP \sin FE + \sin PF \,.\, \sin DE + \sin FD \,.\, \sin PE = 0.$$

Therefore

$$\frac{\sin FE}{\sin FD}\frac{\sin DP}{\sin PE} + \frac{\sin DE}{\sin FD}\frac{\sin PF}{\sin PE} + 1 = 0,$$

whence

$$\frac{\sin ACF}{\sin FCB}\frac{\sin BCP}{\sin ACP} + \frac{\sin DAC}{\sin BAD}\frac{\sin BAP}{\sin CAP} + 1 = 0.$$

But (20)

$$u\frac{\sin ACP}{\sin ACG}=v\frac{\sin BCP}{\sin BCG}, \text{ and } v\frac{\sin BAP}{\sin BAG}=w\frac{\sin CAP}{\sin CAG}.$$

Therefore, putting

$$\frac{\mathrm{u}}{\mathrm{v}}=\frac{\sin ACF}{\sin FCB}\,\frac{\sin BCG}{\sin ACG}, \text{ and } \frac{\mathrm{w}}{\mathrm{v}}=\frac{\sin DAC}{\sin BAD}\,\frac{\sin BAG}{\sin CAG},$$

$$\mathrm{u}u+\mathrm{v}v+\mathrm{w}w=0.$$

Let the great circle EF pass through the poles $h\,k\,l$, $p\,q\,r$, not being opposite extremities of a diameter of the sphere.

Then $\quad \mathrm{u}h+\mathrm{v}k+\mathrm{w}l=0$, and $\mathrm{u}p+\mathrm{v}q+\mathrm{w}r=0$.

These two equations are satisfied by making

$$\mathrm{u}=kr-lq, \qquad \mathrm{v}=lp-hr, \qquad \mathrm{w}=hq-kp.$$

The great circle passing through the poles $h\,k\,l$, $p\,q\,r$ will be called a zone-circle, and will be denoted by the symbol u v w, or by any three integers in the same proportion.

Condition that a pole may be in a zone-circle.

22. It appears from (21) that when the zone-circle u v w passes through the pole $u\,v\,w$, we have

$$\mathrm{u}u+\mathrm{v}v+\mathrm{w}w=0.$$

Any integral values of u, v, w that satisfy this equation are the indices of a pole in the zone-circle u v w, and any integral values of u, v, w that satisfy it are the indices of a zone-circle passing through the pole $u\,v\,w$.

The intersections of any two zone-circles are poles.

23. Let the zone-circles h k l, p q r intersect in the points P, P'. If it be possible, let P be the pole $u\,v\,w$. Then (22)

$$\mathrm{h}u+\mathrm{k}v+\mathrm{l}w=0, \text{ and } \mathrm{p}u+\mathrm{q}v+\mathrm{r}w=0.$$

These equations are satisfied by making

$$u = \mathrm{kr} - \mathrm{lq}, \quad v = \mathrm{lp} - \mathrm{hr}, \quad w = \mathrm{hq} - \mathrm{kp}.$$

It is evident that u, v, w are integers, therefore P is the pole $u\,v\,w$, and P' is the $\bar{u}\,\bar{v}\,\bar{w}$.

Relation between the arcs AK, BK, CK, K being a pole of the zone-circle EF.

24. Let u v w be the symbol of the zone-circle EF, and let K be the pole of the zone-circle EF nearest to C. Then

$$\cos AK = -\sin AE \sin E, \quad \cos BK = -\sin BD \sin D,$$

$$\cos CK = \sin CD \sin D = \sin CE \sin E.$$

The symbol of D is $0\,\mathrm{w}\,\bar{\mathrm{v}}$, and the symbol of E is $\mathrm{w}\,0\,\bar{\mathrm{u}}$. Therefore (20)

$$\frac{\cos AK}{\cos CK} = -\frac{\sin AE}{\sin CE} = -\frac{\sin AB \sin ABE}{\sin BC \sin CBE} = \frac{\mathrm{u}\gamma}{\mathrm{w}\alpha},$$

and
$$\frac{\cos BK}{\cos CK} = -\frac{\sin BD}{\sin CD} = -\frac{\sin AB \sin BAD}{\sin CA \sin CAD} = \frac{\mathrm{v}\gamma}{\mathrm{w}\beta}.$$

Whence

$$\frac{\alpha}{\mathrm{u}} \cos AK = \frac{\beta}{\mathrm{v}} \cos BK = \frac{\gamma}{\mathrm{w}} \cos CK.$$

Anharmonic ratio of four zone-circles passing through one pole.

25. In fig. 9 let A, B, C be the poles 1 0 0, 0 1 0, 0 0 1 respectively; KP the zone-circle e f g intersecting the zone-circles CA in M; KR the zone-circle p q r intersecting the zone-circle BC in N; Q the pole $h\,k\,l$; S the pole $u\,v\,w$. Let the zone-circles KQ, KS intersect the zone-circle MN in T, V; also let $\zeta\,\eta\,\theta$, $\phi\,\chi\,\psi$ be the symbols of T, V respectively. Then (21), (23) the symbol of M will be $\mathrm{g}\,0\,\bar{\mathrm{e}}$, the symbol of N will be $0\,\mathrm{r}\bar{\mathrm{q}}$, the symbol of MN will be er gq gr, the symbol of K will be fr − gq gp − er eq − fp, and the symbol of KQ will be

$$k\,\mathrm{eq} - k\,\mathrm{fp} - l\,\mathrm{gp} + l\,\mathrm{er},$$

$$l\,\mathrm{fr} - l\,\mathrm{gq} - h\,\mathrm{eq} + h\,\mathrm{fp},$$

$$h\,\mathrm{gp} - h\,\mathrm{er} - k\,\mathrm{fr} + k\,\mathrm{gq}.$$

Hence (23)

$$\zeta = g\,(gq - fr)\,(ph + qk + rl),$$
$$\eta = r\,(er - gp)\,(eh + fk + gl).$$

In like manner

$$\phi = g\,(gq - fr)\,(pu + qv + rw),$$
$$\chi = r\,(er - gp)\,(eu + fv + gw).$$

But $\dfrac{\sin PKQ}{\sin RKQ}\,\dfrac{\sin RKS}{\sin PKS} = \dfrac{\sin MT}{\sin NT}\,\dfrac{\sin NV}{\sin MV}$

$$= \frac{\sin ACT}{\sin BCT}\,\frac{\sin BCV}{\sin ACV} = \frac{\eta}{\zeta}\,\frac{\phi}{\chi}.$$

Therefore

$$\frac{\sin PKQ}{\sin PKS}\,\frac{\sin RKS}{\sin RKQ} = \frac{eh + fk + gl}{eu + fv + gw}\,\frac{pu + qv + rw}{ph + qk + rl}.$$

Anharmonic ratio of four poles in one zone-circle.

26. Let the zone-circle QS meet the zone-circle KP in P, and the zone-circle KR in R. Then, since the anharmonic ratio of the points P, Q, R, S is the same as that of the arcs KP, KQ, KR, KS (Tract 16),

$$\frac{\sin PQ}{\sin PS}\,\frac{\sin RS}{\sin RQ} = \frac{eh + fk + gl}{eu + fv + gw}\,\frac{pu + qv + rw}{ph + qk + rl}.$$

Sign of the expression forming the right-hand side of the final equations in (25) *and* (26).

27. In (25) the left-hand side of the final equation may be replaced by its equivalent

$$\frac{\cot PKS - \cot PKR}{\cot PKQ - \cot PKR},$$

and in (26) it may be replaced by

$$\frac{\cot PS - \cot PR}{\cot PQ - \cot PR}.$$

From the form of these expressions it is manifest that they are positive, and therefore also the expression forming the right-hand side of the equations in both cases, except when one only of the zone-circles KP, KR lies between Q and S.

Position of any Pole in each of the six Systems of Crystallization.

Position of any pole in the cubic system.

28. In this system the arcs joining every two of the poles 100, 010, 001 are quadrants, and the arcs joining the pole 111 and each of the poles 100, 001, 001 are all equal. Let A, B, C, O, P be the poles 100, 010, 001, 111, hkl respectively. Then BC, CA, AB are quadrants, $OA = OB = OC$, and the right angles A, B, C are bisected by OA, OB, OC. Hence the equations in (20) become

$$\tan BAP = \frac{l}{k}, \quad \tan CBP = \frac{h}{l}, \quad \tan ACP = \frac{k}{h}.$$

But

$$\tan AP = \tan ACP \sec BAP,$$
$$\tan BP = \tan BCP \sec ABP,$$
$$\tan CP = \tan CBP \sec ACP.$$

Whence

$$(\cos AP)^2 = \frac{h^2}{h^2 + k^2 + l^2},$$
$$(\cos BP)^2 = \frac{k^2}{h^2 + k^2 + l^2},$$
$$(\cos CP)^2 = \frac{l^2}{h^2 + k^2 + l^2}.$$

Position of any pole in the pyramidal system.

29. In this system the arcs joining the poles 100, 010, 001 are quadrants, and the arcs joining the pole 111 and each of the poles 100, 010 are equal. Let A, B, C, G, P be the poles 100, 010, 001, 111, hkl respectively. Then BC, CA AB are quadrants, and $AG = BG$. Consequently $BAG = ABG$, and $ACG = BCG$. The arc BG intersects CA in the pole 101. Putting the arc 001, $101 = E$, and observing that the angles A, B, C are right angles, the equations in (20) become

$$\tan BAP = \frac{l}{k} \cot E, \quad \tan ABP = \frac{l}{h} \cot E, \quad \tan ACP = \frac{k}{h}.$$

$$\cot AP = \frac{h}{k}\cos BAP = \frac{h}{l}\tan E\cos CAP,$$

$$\cot BP = \frac{k}{l}\tan E\cos CBP = \frac{k}{h}\cos ABP,$$

$$\cot CP = \frac{l}{h}\cot E\cos ACP = \frac{l}{k}\cot E\cos BCP,$$

$$(\tan CP)^2 = \frac{h^2+k^2}{l^2}(\tan E)^2.$$

The arc E may be taken for the element of the crystal.

Position of any pole in the rhombohedral system.

30. In this system the arcs joining every two of the poles 100, 010, 001 are all equal, and the arcs joining the pole 111 and each of the poles 100, 010, 001 are all equal.

In fig. 10, let A, B, C, O be the poles 100, 010, 001, 111 respectively; P the pole hkl. Let the zone-circles OA, OB, OC meet the zone-circles BC, CA, AB in D, E, F. Then, since $BC = CA = AB$ and $OA = OB = OC$, it is evident that BC, CA, AB are bisected in the points D, E, F; that $OD = OE = OF$; that the angles at D, E, F are right angles; that the symbols of D, E, F are 011, 101, 110 respectively; and that the six angles having their apices in O are each of 60°.

The symbol of OA is $01\bar{1}$, and that of OB is $10\bar{1}$. Therefore (27),

$$\frac{\cot AOP - \cot AOB}{\cot AOF - \cot AOB} = \frac{h-l}{k-l}.$$

But $AOB = 120°$, $AOF = 60°$. Therefore $\tan AOB = -\sqrt{3}$, $\tan AOF = \sqrt{3}$. Hence

$$\tan AOP = \frac{(k-l)\sqrt{3}}{2h-k-l}.$$

In like manner

$$\tan BOP = \frac{(l-h)\sqrt{3}}{2k-l-h},$$

and

$$\tan COP = \frac{(h-k)\sqrt{3}}{2l-h-k}.$$

Hence,

$$\cos AOP = \frac{2h-k-l}{\sqrt{\{2(k-l)^2+2(l-h)^2+2(h-k)^2\}}},$$

$$\cos BOP = \frac{2k-l-h}{\sqrt{\{2(k-l)^2+2(l-h)^2+2(h-k)^2\}}},$$

$$\cos COP = \frac{2l-h-k}{\sqrt{\{2(k-l)^2+2(l-h)^2+2(h-k)^2\}}}.$$

Let the zone-circle OP meet the zone-circle CA in H and the zone-circle AB in I. The symbol of I will be $h-l,\ k-l, 0$. The symbol of OB is $1\,0\,\bar{1}$, and that of CA will be $0\,1\,0$. Therefore (27),

$$\frac{\cot OP - \cot OH}{\cot OI - \cot OH} = \frac{k}{k-l}.$$

Let $OA = D$. Then

$$\tan OE = \cos 60^\circ \tan OA = \tfrac{1}{2}\tan D;$$

$$\cot OH = \cot OE \cos EOP$$

$$= 2\cot D \frac{l+h-2k}{\sqrt{\{2(k-l)^2+2(l-h)^2+2(h-k)^2\}}},$$

$$\cot OI = \cot OF \cos FOP$$

$$= 2\cot D \frac{h+k-2l}{\sqrt{\{2(k-l)^2+2(l-h)^2+2(h-k)^2\}}}.$$

Hence

$$\tan OP = \frac{\sqrt{\{2(k-l)^2+2(l-h)^2+2(h-k)^2\}}}{2h+2k+2l}\tan D.$$

The arc D may be taken for the element of the crystal.

Position of any pole in the prismatic system.

31. In this system the arcs joining any two of the poles $1\,0\,0$, $0\,1\,0$, $0\,0\,1$ are quadrants. Let A, B, C, G, P be the poles $1\,0\,0$, $0\,1\,0$, $0\,0\,1$, $1\,1\,1$, $h\,k\,l$ respectively. Then BC, CA, AB are quadrants.

The arcs AG, BG, CG meet the arcs BC, CA, AB in the poles $0\,1\,1, 1\,0\,1, 1\,1\,0$. Putting $0\,1\,0, 0\,1\,1 = D$, $0\,0\,1, 1\,0\,1 = E$, $1\,0\,0, 1\,1\,0 = F$, and observing that the angles A, B, C are right angles, the equations in (20) become

$$\tan BAP = \frac{l}{k} \tan D,\ \tan CBP = \frac{h}{l} \tan E,\ \tan ACP = \frac{k}{h} \tan F,$$

$$\cot AP = \frac{h}{k} \cot F \cos BAP = \frac{h}{l} \tan E \cos CAP,$$

$$\cot BP = \frac{k}{l} \cot D \cos CBP = \frac{k}{h} \tan F \cos ABP,$$

$$\cot CP = \frac{l}{h} \cot E \cos ACP = \frac{l}{k} \tan D \cos BCP.$$

Any two of the arcs D, E, F may be taken for the elements of the crystal. They are connected by the equation

$$\tan D \,.\, \tan E \,.\, \tan F = 1.$$

Position of any pole in the oblique system.

32. In this system the arc joining the poles $1\,0\,0, 0\,1\,0$, and the arc joining the poles $0\,1\,0, 0\,0\,1$ are quadrants. Let A, B, C, G, P be the poles $1\,0\,0$, $0\,1\,0$, $0\,0\,1$, $1\,1\,1$, $h\,k\,l$ respectively. Let BG, BP meet CA in L, S. Then L will be the pole $1\,0\,1$, and S the pole $h\,0\,l$. The arcs AB, BC are quadrants, and consequently the angles ACB, CAB are right angles. Hence the equations in (20) become

$$\frac{\tan CAP}{\tan CAG} = \frac{k}{l},\ \frac{\sin AL}{\sin CL}\frac{\sin CS}{\sin AS} = \frac{h}{l},\ \frac{\tan ACP}{\tan ACG} = \frac{k}{h}.$$

Putting

$$\tan \theta = \frac{h}{l}\frac{\sin CL}{\sin AL},$$

the arc AS will be given by the equation

$$\tan (AS - \tfrac{1}{2}AC) = \tan \tfrac{1}{2}AC \tan (\tfrac{1}{4}\pi - \theta).$$

But

$$\sin CL = \cot ACG \cot BG, \quad \sin AL = \cot CAG \cot BG,$$
$$\sin CS = \cot ACP \cot BP, \quad \sin AS = \cot CAP \cot BP.$$

Whence

$$\frac{\tan BP}{\tan BG} = \frac{h}{k}\frac{\sin CL}{\sin CS} = \frac{l}{k}\frac{\sin AL}{\sin AS}.$$

The arcs AL, BG, CL may be taken for the elements of the crystal.

Position of any pole in the anorthic system.

33. Let A, B, C, G, P be the poles 1 0 0, 0 1 0, 0 0 1, 1 1 1, $h\ k\ l$ respectively; and let AG, BG, CG meet BC, CA, AB in D, E, F, the poles 0 1 1, 1 0 1, 1 1 0. It is easily seen that

$$\frac{\sin CA}{\sin AB}\frac{\sin CAG}{\sin BAG} = \frac{\sin CD}{\sin BD}, \quad \frac{\sin AB}{\sin BC}\frac{\sin ABG}{\sin CBG} = \frac{\sin AE}{\sin CE},$$
$$\frac{\sin BC}{\sin CA}\frac{\sin BCG}{\sin ACG} = \frac{\sin BF}{\sin AF}.$$

But (20),

$$k\frac{\sin BAP}{\sin BAG} = l\frac{\sin CAP}{\sin CAG}, \quad l\frac{\sin CBP}{\sin CBG} = h\frac{\sin ABP}{\sin ABG},$$
$$h\frac{\sin ACP}{\sin ACG} = k\frac{\sin BCP}{\sin BCG}.$$

Whence

$$\frac{\sin CAP}{\sin BAP} = \frac{k}{l}\frac{\sin AB}{\sin CA}\frac{\sin CD}{\sin BD},$$
$$\frac{\sin ABP}{\sin CBP} = \frac{l}{h}\frac{\sin BC}{\sin AB}\frac{\sin AE}{\sin CE},$$
$$\frac{\sin BCP}{\sin ACP} = \frac{h}{k}\frac{\sin CA}{\sin BC}\frac{\sin BF}{\sin AF}.$$

Putting $\tan\theta$, $\tan\phi$, $\tan\psi$ respectively for the right-hand sides of the preceding equations, we obtain

$$\tan(BAP - \tfrac{1}{2}BAC) = \tan\tfrac{1}{2}BAC\tan(\tfrac{1}{4}\pi - \theta),$$
$$\tan(CBP - \tfrac{1}{2}CBA) = \tan\tfrac{1}{2}CBA\tan(\tfrac{1}{4}\pi - \phi),$$
$$\tan(ACP - \tfrac{1}{2}ACB) = \tan\tfrac{1}{2}ACB\tan(\tfrac{1}{4}\pi - \psi).$$

Whence, knowing the segments BD, CD, CE, AE, AF, BF, the position of P can be found.

Any five of the segments of the sides of ABC may be taken for the elements of the crystal. For five of the segments being known, the remaining segment is given by the equation

$$\sin BD\sin CE\sin AF = \sin CD\sin AE\sin BF.$$

On the Association of Potton Sand Fossils with those of the Farringdon Gravels in a phosphatic deposit at Upware on the Cam; with an account of the Superposition of the Beds, and the significance of the Affinities of the Fossils. By Mr Harry Seeley, F.G.S.

Abstract.

In 1860 the author had traced the Galt by Swaffham fen, west of Wicken into Soham Mere; fossils were then collected and placed in the Woodwardian Museum. But though the beds over the Kimeridge clay and under the Galt are represented in Dr Fitton's section through Upware, they do not appear to have been seen again until the pits were opened for digging nodules of phosphate of lime last year. These have yielded about 120 species of fossils, chiefly mollusca and spunges, with a number of vertebrates. As a whole they recall beds in the same position in the north of Germany; in part they resemble with unexpected closeness the fauna of the Farringdon gravels; while

the resemblance to the fossils of Potton is such that nearly all Potton types of life have already occurred at the Wicken diggings. Potton however is rich in vertebrate remains and in the phosphatic casts of shells; at Wicken the silico-phosphatic concretions are smaller, and the mollusca, &c. for the most part preserve their carbonate of lime shells; moreover at Potton have occurred Cycadoidea microphylla, Cycad cones, cones of Pandanus and three species of Pinites, besides much wood mineralized, sometimes with phosphate of lime, sometimes with silex. At Wicken but little wood has occurred. All these facts appear to demonstrate that, assuming the phosphate beds at these places to be one and the same, then Potton was nearer to the old land, streams from which brought down the animals and plants, than was Wicken. This was assumed. Then it would follow that on these deposits being traced to the south-west they would become fluvio-marine and freshwater, and finally have no representative in that direction because of the interposition of dry land. Also if these remains were brought down by rivers (and they are in the same state of preservation as bones from the Wealden), the river banks on ceasing would become continuous with the sea shore of the land, along which would be distributed fragments of the rocks which formed the cliffs in those days, as well as rolled bones.

Such a pebble bed is found, and with it are mixed the nodules of phosphate of lime, the casts of shells, the sand nodules concreted with phosphate of lime, and most of the reptilian remains. The pebbles are chiefly old rocks, black slate, Lydian stone, brown hornstone, white and rose quartz, with an occasional fossil from the mountain limestone, notably joints of the column of Poteriocrinus.

And it would also follow that on these deposits being traced out at sea parallel to the shore the pebbles would cease, the sands would become fine and thin, and ultimately be replaced by clay; moreover, assuming a small river to have brought down

the remains of Megalosaurus, Hylæosaurus, Iguanodon, &c. into a sea tenanted with Pliosaurs, and Ichthyosaurs, and Plesiosaurs, &c., then after depositing these heavy bones and their sand, the fine mud would still be carried out to sea, and with it some of the spoils of the land, as in the case of all river deposits.

The sections at Wicken were simple, being ferrous sands with varying courses of nodules of phosphate of lime and pebbles sometimes united into a bed six feet thick, oftener subdivided by intervening sand into two or three beds of from a few inches to a foot or two thick. Under these and not well separated is a thin band rich in calcareous matter, often making it a hard continuous agglomerate; but it is extremely variable, and sometimes disappears in small isolated concretions. These beds, which were spoken of as the Potton sands and Wicken beds, rest at Wicken partly on the white Upware limestone (usually called Coral Rag, but regarded by the author as not older than the lower part of the Kimeridge clay), and partly on a blue clay with Ammonites serratus, and near the top casts of Nucula and other shells, &c. in phosphate of lime. This blue clay the author regarded as Kimeridge clay, though the Ammonites serratus usually occurs lower in the series. Above the sands is the Galt, the actual junction not seen, though there are cracks in the sands a foot or two wide into which the Galt with its characteristic fossils has been squeezed.

The author followed the Wicken beds to Harrimere, near to which place they form the bed of the river, as a hard dark grey fine sand agglomerate of shells and phosphatic nodules. The species were numerous; those collected are in the Woodwardian Museum. This probably represents the lower phosphate bed at Wicken. Up to the old West Water the phosphatic casts of Lucina, Myacites, Cyprina, Ammonites, &c. occurred plentifully in the bed of the river for two miles before its junction with the Cam. At Stuntney the hill is capped with a thin bed of nodules of phosphate of lime, like those at Wicken with similar

fossils. S.E. of High Hill the brown ferrous sands are well seen coming from under the Galt of Soham, which has been bored for 450 feet without being pierced; near the base of the Galt nodules of phosphate of lime and fossils abounded. At Ely these beds are variable. At the Gallows Pits the rock is so calcareous as to split with a crystalline fracture. The old walls of the city are of a fine grit conglomerate with occasional nodules of phosphate of lime, casts of fossils, and bones still to be seen in the blocks; while the Cemetery is on sand, said by the gravedigger to be about 12 feet thick with indurated beds in the middle. In Roswell Hole the conglomerate rock was at the bottom and sand above; here from the rock were obtained about six species of mollusca. At Wilburton a phosphatic band with jaws of Edaphodon occurs near the top of the brown sands. At Haddenham the sands 30 feet thick rest on the upturned and eroded clay [Kimeridge]. In the middle were found three small phosphatic concretions. At Aldreth no concretions were seen. Mr Westrupp states that in dredging the Cam he finds the bottom to be a mixture of gravel and galt between Bottisham sluice and Swaffham sluice; and that at a place near Bottisham sluice called Calves Flat the bed of the river is a hard ferruginous bed with a hard bed below, mixed with gravel and galt. It is highly probable that these are the phosphate beds. Mr Westrupp also states that gravel exists under the whole of Isleham Fen*.

At Downham Market the top of the Kimeridge clay contains small sand concretions and phosphatic casts of shells with green grains in them, resembling in species and preservation those from the sands at Wicken.

At Hunstanton the phosphatic concretions are numerous but chiefly fragments of casts of Ammonites Deshaysii, and about

* Since this paper was read the author has seen this phosphate bed under ferrous sands and graduating into the clay on which it rested at Impington. It appeared to be in situ.

a dozen other mollusca. They occur very near the bottom of the sands, heretofore called the Carstone.

These facts do not demonstrate the position in the geological sequence of the Potton and Wicken beds.

In the south of England the beds between the Kimeridge clay and the galt are

(Lower Green or) North-Down sands*,
Wealden series, }
Purbeck series, }
Portland series,

and to represent the whole of these there are in this district only the Potton sands. These sands in the middle of England have generally been referred to some *portion* of the series. Thus Smith called them the sand of the Portland rock. Conybeare, who instituted the Ironsand group, supposed the Portland series as well as the Greensand to thin away northward, and so put these deposits into his Ironsands; and Fitton, who instituted the Lower Greensand, supposed the Portland, the Purbeck, and the Weald, to thin off to the north, and threw these beds into the Lower Greensand.

The author then detailed at length the physical characters of the beds between the Kimeridge clay and the Galt in all the English sections, and arrived at the conclusion that the period of elevation indicated by the pebble beds of Potton and Wicken was identical with the period of elevation indicated in the south of England by the Purbeck and Wealden group, the marine equivalents of which would be thin.

And in this district the author supposed the upper part of the clay called Kimeridge to be only the necessary clay representative of beds which to the south are sands.

Under these circumstances it was thought that the old

* The North Downs give the types of the divisions of the so-called Lower Greensand adopted by the Geological Survey.

nomenclature of Cretaceous and Oolite, as divided by Professor Forbes, could not now be sustained. And the author proposed the following physical groups as more true and convenient for English geology.

Mr Seeley's divisions.			Old names.
Cretaceous series.	Chalk		Cretaceous.
	Upper Greensand		
	Galt		
Psammolithic series.	North Down-Sand or Lower Greensand		
	Wealden	Potton sands and	
	Purbeck	Wicken beds	Upper Oolite.
	Portland		
Pelolithic series.	Kimeridge Clay		
	Coral Rag and Ampthill Clay		Middle Oolite.
	Oxford Clay		
Oolitic series.	Great Oolite, &c.		Lower Oolite.
	Inferior Oolite, &c.		
	Lias		
	Trias		

In a large number of cases, the fossils, vertebrate and invertebrate, both those with the shells preserved and the casts, had marked affinity with fossils from lower beds. The author endeavoured to account for this by the conditions of physical geography accumulating in one area portions of the fauna from several successive periods, though admitting that some species were probably derived from the denudation of adjacent inferior strata.

Professor Sedgwick made some remarks upon the position and relation of the English greensands, and expiated upon the richness of the deposits of this era near Cambridge and the importance of the Upware fossils.

Professor Liveing asked whether Mr Seeley meant that the Wealden was denuded at Shotover and its neighbourhood to form these beds, and enquired where he supposed this Wealden land to be.

Mr Seeley said that he had not meant to imply that the Wealden land was denuded to form these deposits, but that the animals therein were living in the surrounding seas or were brought down from it by streams into them; as for the position of the land it was a difficult question, and he was not yet prepared to give a positive opinion, but he believed it to be somewhere to the S.E. of Britain.

Mr Walker (Sidney) asked Mr Seeley whether the presence of Gryphæa dilatata in these beds was not an objection to his theory, and expressed a difference of opinion as to the mode of formation of the phosphatic nodules.

Mr Seeley replied that he thought the presence of a supposed species of oyster a matter of little moment, as the specific distinctions were of such small value.

(PART VI.)

October 28, 1867.

Professor CHALLIS (VICE-PRESIDENT) in the Chair.

The following were elected Officers of the Society for the ensuing year.

President.

Professor SELWYN.

Vice-Presidents.

Professor HUMPHRY.
Professor STOKES.
Professor CAYLEY.

Treasurer.

Rev. W. M. CAMPION.

Secretaries.

Professor C. C. BABINGTON.
Professor LIVEING.
Rev. T. G. BONNEY.

New Members of the Council.

The Rev. H. W. COOKSON, D.D.
Professor CHALLIS.
Professor MILLER.
Professor ADAMS.

On Skew Surfaces. By Professor CAYLEY, F.R.S.

November 11, 1867.

The PRESIDENT (Professor SELWYN) in the Chair.

The following new Fellow was elected:

C. K. ROBINSON, D.D., Master of *St Catharine's College.*

(1) *On the Use of a Camera Lucida Prism in measuring distances and levelling. By* Professor MILLER, F.R.S.

(2) *On a Series of Elevated Sea Terraces on Hampsfell, near Cartmel, Lancashire. By* F. A. PALEY, M.A.

THIS paper gave a description of a remarkable plateau of mountain limestone surmounting the hill, which is something under 800 feet high, that separates Cartmel from the village of Grange, on the Lancaster and Furness Railway. The marks of sea action,—or at least, of aqueous action,—were very distinct over the whole of this plateau of naked rock; and the resemblance between them and the wave-scored rocks at the level of the present sea is so striking, that the author expressed an opinion that the whole hill had been upheaved within a comparatively recent period. The evidences on which he chiefly relied were, first, a large number of limestone and slate boulders still covering the hill, and evidently the result of glaciers or ice-floes in a glacial sea, and secondly, a well-defined sea-terrace, rising with scarcely any break from the present sea below Grange to the crown of the Hampsfell.

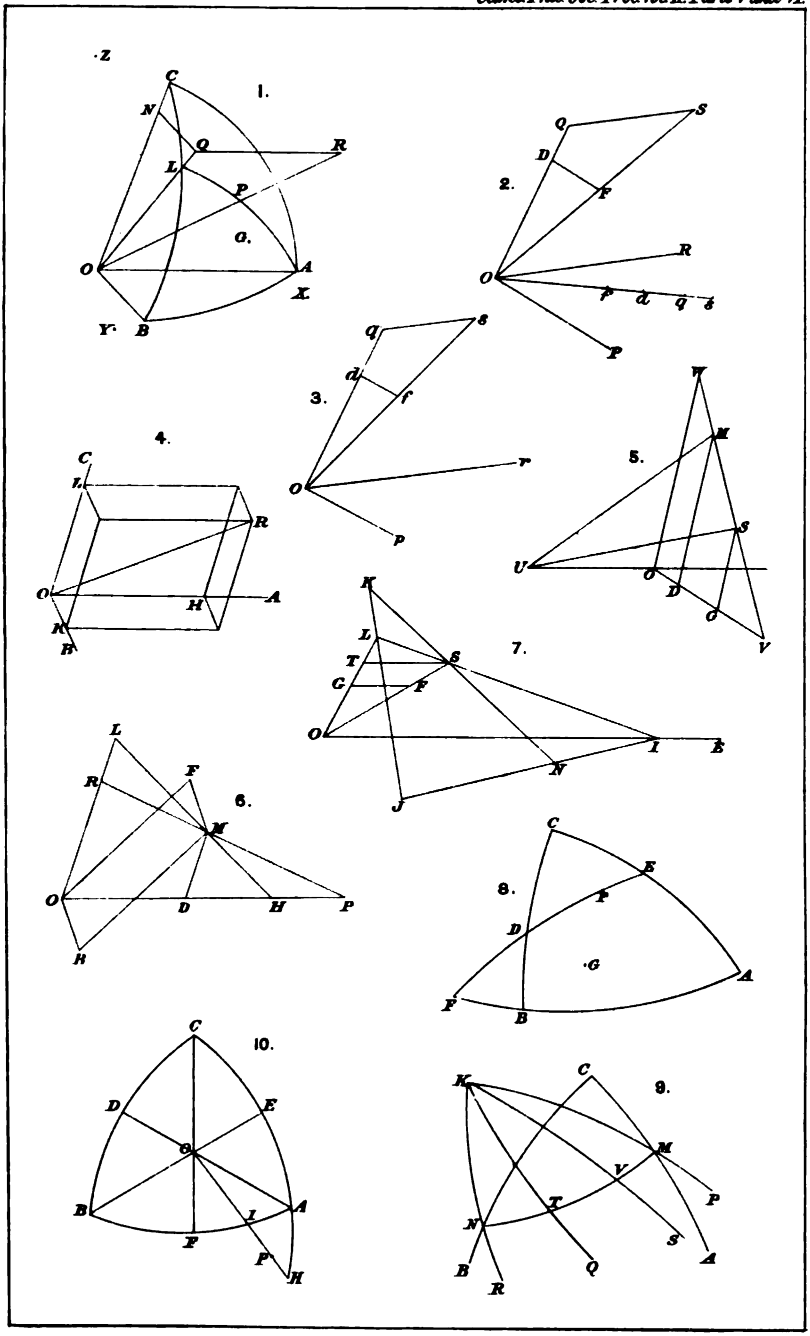
A succession of steps or rocky flats on the western side of the hill having the several floors wave-marked in the same manner, and each terminated by a low wall or cliff, were referred by the writer to successive upheavals of the hill subsequent to the deposition of the boulders, which he thought most probably

were dropped when the present hill was submerged a considerable depth below the sea.

Mr Bonney agreed with Mr Paley in considering that the west coast of England and Wales had been submerged beneath the sea at no very distant geological epoch, and gave instances of the occurrence of valleys of marine erosion and sea cliffs in Wales and Cheshire, noticing similar terraces on the great Ormes-head. He, however, doubted whether the scoring and furrowing of the rock could without hesitation be referred to the action of the waves. He had seen many instances of a similar structure in places where glaciers had modified the surface after its upheaval from the sea. This in his opinion had been the case in parts of the Ormeshead, and had certainly been so in many of the Alpine limestone districts, as for example in the Italian Tyrol, on the range of the Parmelan near Annecy, and near the Gemmi Pass in the western Oberland. He believed that the structure was due to the nature of the rock and the way in which it yielded to the agents of subaerial denudation. Although no doubt boulders had often been transported by ice-floes, yet he thought that in the neighbourhood of the Lake District glaciers had also aided.

Mr Paley said that he had very carefully examined the markings on the Fell rocks, and thought that they corresponded so exactly with those on the shore rocks in Morecambe Bay, that the causes which produced them must be identical. As the boulders were usually large and isolated, he thought that they were more probably transported by floes when the country was under water.

Professor Liveing concurred with Mr Bonney in thinking these surface-markings to be due rather to the 'habit' of the limestone rock, and instanced what, from Mr Paley's description, he thought were similar markings on the inland limestone districts in the Yorkshire Fells.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Mr PRITCHARD (President of the Royal Astronomical Society) presented some copies of a new star-chart and of a chart of the November meteors (1866), and made some remarks in explanation of them, and on the mode of observing these meteors, especially pointing out the importance of counting them, for that by this means the form of the orbit had been roughly inferred. He expected that the shower would take place from about four to six on Thursday morning, Nov. 14th.

November 25, 1867.

The PRESIDENT (Professor SELWYN) in the Chair.

The following new Fellows were elected:

C. J. LAMBERT, B.A., *Pembroke College.*
G. PIRIE, B.A., *Queens' College.*

On Aristophanes. By the Rev. W. G. CLARK, M.A. (Public Orator.)

Mr Clark gave a description of the Ravenna MS. of Aristophanes, written in the eleventh century, and traced its history to the library of Urbino. He exhibited photographs of two pages, and described its importance and value as compared with the other MSS. at Venice, Florence, Rome, Modena and Paris.

(PARTS VII—X.)

PROCEEDINGS

OF THE

Cambridge Philosophical Society.

10

Cambridge:
PRINTED BY C. J. CLAY, M.A.
AT THE UNIVERSITY PRESS.

February 17, 1868.

The PRESIDENT (PROFESSOR SELWYN, D.D.) in the Chair.

The following new Fellows were elected:

C. TROTTER, M.A., *Trinity*.
T. W. DANBY, B.A., *Downing*.

The President (Professor Selwyn) presented to the Society a volume containing two tracts on Coinage, one by Nicholas Oresmus; the other by Copernicus; and made a few remarks upon it.

Communications to the Society:

(1) *On the distinction of Subjective and Objective Probability.* *By* C. J. MONRO, M.A.

(2) *On the existence in the Pterodactyle pelvis of bones like those termed marsupial in the Monotremata, and other indications of mammalian affinities of Pterodactyles.* *By* H. SEELEY, F.G.S.

The author exhibited a drawing of the femur of a Dimorphodon, and pointed out the close resemblance which it presented in form to that of a mammal; he also exhibited drawings of the pelvis of the same Pterodactyle, and of those of Iguana, Chelidon, and Echidna: shewing that the first had much more affinity with the last than with the other two; he also noticed some peculiarities of the brain of the same; whence he concluded that the mammalian affinities of the Pterodactyle were too marked to be entirely overlooked.

March 2, 1868.

The PRESIDENT (PROFESSOR SELWYN, D.D.) in the Chair.

Communication to the Society:

On some phenomena of the weathering of rocks illustrating the value of material denudation as a geological agent. By D. T. ANSTED, M.A., F.R.S.

(This paper is printed in the Society's Transactions.)

March 16, 1868.

The PRESIDENT (PROFESSOR SELWYN, D.D.) in the Chair.

The following new Fellows were elected:

W. C. GREEN, M.A., *King's.*
E. S. SHUCKBURGH, B.A., *Emmanuel.*
C. A. M. FENNELL, B.A., *Jesus.*
E. HILL, B.A., *St John's.*
E. H. PALMER, B.A., *St John's.*

Communications to the Society:

(1) *On Propositions numerically definite. By the late* Professor BOOLE (*communicated by* Professor DE MORGAN).

(This paper is printed in the Society's Transactions.)

(2) *On the Curvæ lintearia of John Bernoulli. By* T. POTTER, M.A.

April 27, 1868.

PROFESSOR HUMPHRY, M.D., VICE-PRESIDENT, in the Chair.

The following new Fellows were elected:

T. WEBSTER, M.A., *Trinity.*
B. ANNINGSON, B.A., *Caius.*

Communication to the Society:

On the Elevation of Mountains by lateral pressure; its causes and amount, with speculations upon the origin of volcanic action. By O. FISHER, M.A.

(This paper is printed in the Society's Transactions.)

May 11, 1868.

PROFESSOR HUMPHRY, M.D., VICE-PRESIDENT, in the Chair.

Communication to the Society:

A description of a new Celestial Globe by Syed Rujjub Allie. By E. H. PALMER, B.A., *St John's College.*

May 25, 1868.

The PRESIDENT (PROFESSOR SELWYN, D.D.) in the Chair.

The following new Fellows were elected:

J. F. MOULTON, B.A., *Christ's.*
J. B. HASLAM, B.A., *St John's.*

The Treasurer made his financial statement; his accounts were passed; the Museum account was also audited and passed; and he was authorized to transfer the balance from it to the general account of the Society; and the thanks of the Society were returned to him.

Communications to the Society:

(1) *A supplement to a paper on the discontinuity of arbitrary constants which appear in divergent developments. By* Professor STOKES, M.A., F.R.S.

(This paper is printed in the Society's Transactions.)

(2) *On Transmutation of Species and the Darwinian theory of it. By* Professor HUMPHRY, M.D., F.R.S.

This paper was an elaborate review of Mr Darwin's theory, in which the author made a few remarks on the theological aspect of the question, and commented upon those parts of it which he considered to be difficulties in it—such as the fact that most of the variations in species observed by Mr Darwin had been under artificial conditions and the absence of transitional forms.

In the debate which followed, Mr N. GOODMAN and the PRESIDENT, Professor SELWYN, took part.

October 26, 1868 (Annual General Meeting).

The PRESIDENT (PROFESSOR SELWYN, D.D.) in the Chair.

The following were elected Officers of the Society for the ensuing year.

President.

Rev. Prof. SELWYN.

Vice-Presidents.

Dr HUMPHRY.
Rev. W. G. CLARK.
Prof. CAYLEY.

Treasurer.

Rev. W. M. CAMPION.

Secretaries.

Prof. C. C. BABINGTON.
Prof. LIVEING.
Rev. T. G. BONNEY.

New Members of the Council.

Rev. Dr KENNEDY.
Dr PAGET.
Prof. STOKES.

Communications to the Society:

(1) *On Captain Caron's Zirconia Light;* (2) *On the Chalumeau à platine of MM. Bourbouse et Wiesnegg. By* Professor MILLER, F.R.S.

Professor MILLER gave at first a brief sketch of the uses of lime and zirconia, and pointed out the difficulties caused by the lime melting away at a high temperature. The superiority of zirconia, as has been recently demonstrated by Capt. Caron, is that its light is very brilliant, and it is absolutely insoluble under the gas blow-pipe. Zirconia is found in Auvergne, America, and Russia; though it is not very abundant. Professor Miller described an exhibition of this light near Paris, the result of which was very satisfactory.

Professor MILLER then gave a brief description of the *Chalumeau à platine.*

Professor LIVEING spoke of a contrivance by which the inconvenience of the lime melting had been rendered less; also he said that zircon was common in the zircon syenite of the South of Norway, so that he did not expect any difficulty in obtaining the material.

(2) *On the Composition of the Mortar of the old Church of Little Ellingham, Norfolk. By* Professor LIVEING.

The object of this communication was to confirm some observations made by Mr Spiller, and laid before the British Association at Norwich this year, to the effect that the hardening of ordinary mortar is chiefly due to the conversion of the lime into

carbonate, and that there is little or no chemical action between the lime and sand, and also to shew that in situations exposed to moisture, such as near the ground, where it is well known that mortar usually sets very hard, the lime in the interior of a thick wall may be almost entirely converted into carbonate.

The old church of Little Ellingham was this year pulled down in consequence of a fire which destroyed the roof and otherwise damaged the building, and the mortar analysed was taken from the south wall while in process of demolition, at a spot about a foot above ground and about the same distance from either face of the wall, which was about two feet thick. This mortar was quite unaffected by the fire. The walls were of flint, and Mr Healey of Bradford, the architect of the new church, considers them to have been built about the year 1400 A.D. An inferior limit to the age of the building is fixed by extant records relating to the insertion of a particular window in 1469, which window remained until the fire this year, so that the mortar may be assumed to have been 400 years old at the least. The following are the results of the analysis:—

$CaCO^3$, &c. soluble in HCl......	27·49
Sharp siliceous sand	72·51
	100·00

The part soluble in HCl gave in 100 parts

CO^2	39·21
SiO^2..................	·83
SO^3	1·53
CaO..................	52·64
MyO	·95
Na_2O	·05
Fe^2O^3, Al^2O^3	·82
H^2O	5·10
	101·13

The quantity of soluble silica is not more than might be expected in any specimen of lime and cannot be attributed to the action of the sand on the lime. The quantity of BC^3 is equivalent to 49·9 of CaO, and the remainder of the lime appeared to be partly in the form of sulphate, and probably partly in the form of $CaCO^3$, CaH^2O^2, since treatment of the mortar with water failed to extract any appreciable quantity of hydrate. The water is more than would be combined in the sulphates, so that perhaps a small quantity of the $CaCO^3$ was in the hydrated form, such as is precipitated spontaneously from a solution of CaH^2O^2 in syrup.

November 9, 1868.

The PRESIDENT (PROFESSOR SELWYN, D.D.) in the Chair.

The following new Fellows were elected:

H. JACKSON, M.A., *Trinity.*
T. DALE, M.A., *Trinity.*
G. H. DARWIN, B.A., *Trinity.*

In noticing the presents, the President called attention to a beautiful engraving of the nebula in the sword-handle of Orion, presented to the Society by Lord Rosse.

The Astronomer Royal made some remarks on the engraving, calling attention to the fact that it was the first in which dark stood for dark sky and light for the brighter parts.

Communications to the Society:

(1) *On the factorial resolution of* $x^{2n} - 2\cos n\alpha + \frac{1}{x^{2n}}$.

By the ASTRONOMER ROYAL.

This was a proof of the well-known resolution by the use of ordinary algebra, without the aid of imaginary quantities, against the use of which as a mode of education the author of the paper spoke strongly.

Professor Challis said that though impossible quantities might not be very good instruments of education, they were, in higher investigations, absolutely necessary, and true in the strictest sense.

Professor Cayley believing that the theory of impossible quantities was absolutely true, did not wish it to be excluded from the University studies or placed in a secondary position.

Professor Adams, admitting that imaginary quantities might be used without proper logic, considered that though undiscovered there must be a logic discoverable, and thought the Astronomer Royal's proof of the Theorem not the natural proof, since it was so complicated. He also held that the proof of imaginary quantities could be, and, in some cases had been rendered strictly logical.

(2) *On some Porismatic Problems. By* W. K. CLIFFORD.

The PROBLEM:—To draw a polygon of a given number of sides, all whose vertices shall lie on one given conic, and all whose sides shall touch another given conic: is either not possible at all, or possible in an infinity of ways. This remark, originally made by Poncelet, has been shewn by Professor Cayley to depend in a very beautiful manner upon the theory of elliptic functions; and in this way he has proved that an analogous theorem holds good wherever a (2, 2) correspondence exists: that is to say, whenever two things are so related that to every position of either there correspond two positions of the other. Two points x, y for instance, in a conic U, which are connected by the relation that the line xy touches a second conic, V, have a correspondence of this kind: for if the point x be taken arbitrarily, two tangents can be drawn from it to V, determining two positions of y: and conversely, the point y being fixed determines two positions of x. The theorem is then that in a (2, 2) correspondence there is either no closed cycle of a given order, or an infinite number. In the present com-

munication I propose first to prove this result by the method of correspondence alone, and then to extend the proof to higher orders of correspondence.

In a (2, 2) correspondence there are 4 ($= 2 + 2$) united points, that is to say, four points each of which coincides with one of its correspondents. In fact, if two numbers x and y are connected by an equation of the second degree in each of them, then when we make x and y coincide, there results an equation of the fourth degree (Chasles, *Comptes Rendus*, 1864). I call these united points the points a. Each point a has one of its correspondents coinciding with it; it has also another correspondent b. Each point b again has another correspondent c, and so on. There are also four points α, each of which is such that its two correspondents coincide in a point β. For let q be a correspondent of p and r a correspondent of q; then the relation between p and r is a (2, 2) correspondence (since to each position of p there are two positions of r and *vice versa*), and therefore has four united points, viz. the points β. Each of these points β has another correspondent γ, and so on. We have thus two series of points, $abcd \ldots \alpha\beta\gamma\delta \ldots$ each letter indicating a set of four generally distinct points.

Let us now endeavour to obtain a closed cycle of an odd order: for distinctness' sake we will try to draw a pentagon inscribed in one conic, U, and circumscribed to another, V. Start with a point x on the outer; pass to one of its correspondents, y; y has another correspondent, z; from z we go to u, from u to v, from v to w. If now w were the same point as x, we should have succeeded in our object. But the relation between w and x is a (2, 2) correspondence, for we might have started from x in either of two directions. The united points of this correspondence should therefore apparently give solutions of our problem.

But these united points are no other than the four points c. For starting with one of these, we get the cycle $cbaabc$, which

is a sufficient solution of the correspondence problem last enunciated. But it is *not* a solution of the original problem: for the series will go on *cbaabcde*...and not repeat itself, so that the points *cbaab* do not form a *proper* in-and-circumscribed pentagon. Thus the problem is in general impossible. If however there is any proper solution, the equation of the fourth degree (which determines the improper solutions) will have more than four roots, and will therefore be identically satisfied by any number whatever; so that whatever point x we start with, the point w will come to coincide with it.

Precisely similar reasoning is applicable to the cycles of an even order. Thus e.g. for a quadrilateral we get the four improper solutions $\gamma\beta\alpha\beta$ got by starting from the points γ. I pass to the consideration of correspondences of higher orders.

In an (r, r) correspondence there are

$$2r \text{ united points } a;$$

their remaining correspondents form

$$2r(r-1) \text{ points } b;$$

to these again correspond

$$2r(r-1)^2 \text{ points } c, \text{ and so on.}$$

Similarly there are

$$2r(r-1) \text{ points } \alpha,$$

each of which is such that two of its correspondents coincide; viz. these are

$$2r(r-1) \text{ points } \beta,$$

to which also correspond

$$2r(r-1)^2 \text{ points } \gamma, \text{ and so on.}$$

Now if we attempt to form a closed cycle of the n^{th} order, we shall be led to a correspondence

$$\{r\,.\,(r-1)^{n-1},\ r\,.\,(r-1)^{n-1}\},$$

which has $2r\,.\,(r-1)^{n-1}$ united points. From this number we shall have to subtract the number of improper solutions as given

by our previous reasoning; thus we shall find

$(n = 2m + 1)$, $\{2r (r - 1)^{2m} - 2r (r - 1)^m\}$ proper solutions.

$(n = 2m)$, $\{2r (r - 1)^{2m-1} - 2r (r - 1)^m\}$ proper solutions.

For example, the problem to inscribe on a conic a triangle whose side shall touch a given curve of the third class admits of twelve proper and twelve improper solutions. If the number of proper solutions exceeds this number, the problem becomes porismatic: that is to say, there is an infinite number of solutions.

W. K. C.

Professor Cayley spoke of the importance of Mr Clifford's remarks.

The President exhibited some photographs of the transit of Mercury, taken at Ely, and described the manner in which they were taken.

The Astronomer Royal said that when a small planet approaches the edge of the sun's disk, a black line is observed to shoot out from it, and rapidly thicken out. The cause of this is irradiation, by which the sun appears larger and the planet smaller than they ought, and the true contact with the sun's rim takes place when the black line is first seen; he thought the appearance simply ocular, but wanted further evidence.

Professor Cayley exhibited a model of a certain developable surface.

November 23, 1868.

The PRESIDENT (PROFESSOR SELWYN, D.D.) in the Chair.

New Fellow elected:

A. MARSHALL, M.A., *St John's.*

Communications to the Society:

(1) *On the comparatively recent date of the period when the works of Greek authors were first committed to writing.* *By* C. A. M. FENNELL, M.A.

The following is the Author's abstract of this Paper, which is printed in the Society's Transactions.

FULL statement "That among the Greeks Prose Literature was first committed to writing not earlier than the Persian wars, that is, in Ionia not before 510 B.C., in the rest of Greece not before 490 B.C., and that Metrical literature was first indited several years later, say 450 B.C."

Importance of the subject.

Mr Paley's opinion quoted.

Appeal against allowing prejudices on the subject to influence argument.

The theory based on 1st, strong negative evidence of classical authors; 2nd, inferences from general history; 3rd, inspection of inscriptions; 4th, indications given by the vocabulary.

Examination of evidences given by Herodotus and Thucydides.

Heraclitus the earliest writer mentioned expressly by an extant classical author.

For the Homeric papers, see Mr Paley's paper.

Passages from Dr Thirlwall's History.

Evidences of metrical authors.

History of the progress of the art consistent with the Author's theory. The limits of its application.

Reasons for its tardy development.

The character of the alphabet.

Testimony of inscriptions.

Effect of Persian wars.

Reasons for the priority of Ionian prose literature.

Suggestions as to how the change from the use of memory to that of writing was effected.

Discussion of the reported existence of libraries. Attic literature in Solon's time.

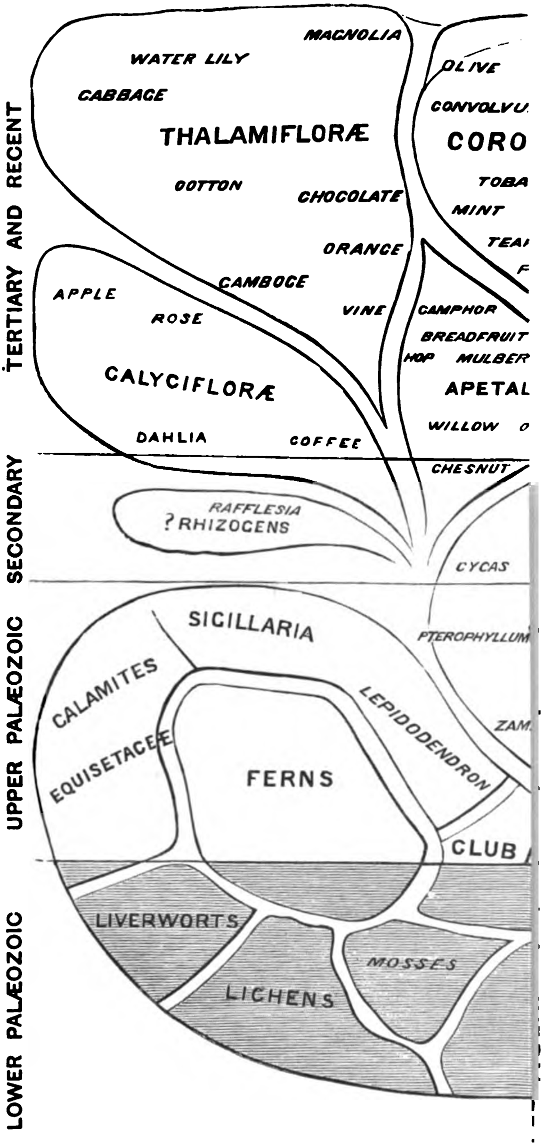

Diagram illustrating Mr

Beginnings of Prose—Genealogies and Theogonies—Philo-
ical treatises—*οἱ λόγοι*.
Origin of the common opinion.
Registration.
Legends ascribing remote antiquity to writing.
Indications given by vocabulary and phraseology.
Concluding remarks on the Greek education and character.

(2) *A note on the resolution of* $x^n + \frac{1}{x^n} - 2 \cos n\alpha$ *into factors.*

MONDAY, *February* 8, 1869.

The PRESIDENT (PROFESSOR SELWYN, D.D.) in the Chair.

Communications to the Society:

(1) *On the Succession of Plant Life upon the Earth.* *By* J. W. SALTER, F.G.S., &c.

In the course of a work on Fossil Botany, I found that I could exhibit in a tabular view a scheme of Plant-life which should combine a natural arrangement of the Vegetable orders with their Geological succession: and I have the pleasure to lay this map before the Society this evening.

I have adopted a prismatic order for the geological colours[1], because that order truly expresses the gradual and harmonious transition of one epoch into another. And since 1848, when I laid this scheme of colouring before the British Association, I have seen no arrangement of geological colours which better expresses the sequence of formations, and their relation one to another. There are no hard lines in the spectrum, and there are no sharp lines of demarcation in the geological history. What is wanting at one place is filled up in another; and, in the succession of plants, as well as of animals, the progress of

[1] The colouring is omitted in the printed copy of the diagram.

discovery only tends to link yet more closely the rocks of each successive period one with the other.

And if, in placing the whole of the flowerless plants of the lower part of the series, I appear to sin against the obvious facts, that most of the *Cryptogamic* orders continue to the present day, I beg it will be understood, that by this arrangement I have only attempted to indicate a *maximum* for each succeeding epoch; and by no means would assert, that the higher tribes had not their roots further back in time than we have yet discovered them. And still less, of course, to assert, contrary to the fact, that branches of the Cryptogamic group do not persist till now,—the humble descendants of what was once the reigning dynasty.

I do not think it necessary to quote Dana, whose pregnant comparison of successive geological epochs with human dynasties must be fresh in the memory of every geological student. The commencement of one ruling caste in the history of men, as of lower organic groups, during the culmination of another, is so natural and familiar to us, that it only needed the genius of that accomplished author to clothe it in appropriate language,—and then to become an accepted axiom in natural history. I therefore make no apology for restricting the Cryptogamia to the *Palæozoic*; or the Flowering plants to the *Neozoic* strata. And I wish the tabular scheme to speak for itself, with only the following remarks:

1. We have, in the early stratified rocks, evidence of the existence of banks of *seaweed* and *nullipore,* only by means of the beds of Graphite and stratified limestone, which occur all over Canada. The origin of these is not doubtful to the chemist, and Mr Sterry Hunt has already claimed them as of Vegetable origin.

2. In the Lower Cambrian rocks of Sedgwick, traces only of low *Algæ* have been found (*Corallines* and other calcareous forms): and in the Silurian, fronds of seaweed, apparently as

highly developed as the *Fucus* of our own coasts, are numerous enough.

3. At the close of the Silurian period, the great tribe of *Club-mosses* made its appearance, in an anomalous form; and thenceforward through the Devonian and Carboniferous eras developed enormously in variety, and of size unknown in modern times. With these, during the same period, *Ferns* in great variety, *Equisetaceæ* of giant size, (*Calamites*) and *Sigillaria*,—a tree, which, related to all the higher *Cryptogamia*, rose almost to a level in structure with the *Cycad.* And here, in the *Coal-measures*, *Cycadeæ* and *Coniferous* trees began to abound. The rank of these amongst flowering plants has been questioned, but no one who considers their manner of growth, imperfect inflorescence, and naked seeds, can doubt that they are inferior to the Oak and the Magnolia,—the rose and the lily of modern times.

4. The botany of the secondary period is less known than it should be. But *Cycads* and *Coniferæ* appear to have risen to their maximum therein; while traces only of a few aquatic *Monocotyledons* are found, to justify us in arranging with these, a few doubtful plants which seem to belong to this order in the higher Coal-measures, but the nature of which is still *sub judice.* I refer to such plants as *Antholithes* and *Pothocites* in the coal; and to the *Naiadaceæ* (pond-weeds, &c.) found sparingly in Oolitic strata.

5. The Cretaceous period abounded in Coniferæ, but included in its upper portion true *Dicotyledons* and *Monocotyledons*, yet the *Iguanodon* must have browsed chiefly on marshy ferns and marsh-loving firs and cypresses, while the Tertiary flora was being ushered in.

6. The Tertiary flora, though capable of much subdivision, so much resembles part of our own forest fauna, that it is needless to speak more fully of it in this short paper. With a distribution of forms wholly unlike what obtains at the present day,

the fruits and stems and leaves found in the London clay and the leaf-beds of the Isle of Wight shew us *Palms* and *Screw-pines*, *Cassia*, *Myrtle*, Leguminous plants in great variety, and very few *Compositæ*: yet the Mediterranean flora and that of the *Miocene* may be well compared: and the oaks and willows, tulip trees, *Rhamni*, and plane trees of America, mingled with Palms and Cinnamons from the tropics, and Proteaceæ from the Cape, are all to be found in Miocene strata; and they abound in the lower Miocene which is so perfectly developed in Switzerland, and which has received the attention of a dozen good European botanists (*Heer*, *Raulin*, &c. &c.).

There has thus been apparently a true "succession of Plant-life" on the globe. And should any one object that the data are too few to furnish a basis for argument, let it be answered, that the data as far as known, agree with those drawn from the much more perfect series of buried animals. And further, that the progress of all true discovery has been this: that the known data should be accepted and reasoned on, until better turn up. And that an hypothesis may be greatly useful, even if founded on scanty data, provided we include nothing that is uncertain—and use it, as Sir H. de la Beche used to say—only as a peg to hang facts upon.

(2) *A sketch diagram of the Passage of the Brachiopoda to the Bivalves and the Bivalve to the Univalve. By* Mr SALTER, F.G.S.

(3) *Note on Crotalocrinus rugosus, a remarkable Crinoid in the Woodwardian Museum. By* Mr SALTER, F.G.S.

(2) and (3) are printed in the Society's Transactions.

Professor BABINGTON in expressing his approbation of the map drawn up by Mr Salter, spoke of the great difficulty of assigning fossil plants to their order and genera, which difficulty was perhaps less in the coal measures.

Professor HUMPHRY complimented Mr Salter upon his clear and able exposition of the relation of the bivalves and univalves, as exemplifying the great principles of modification which may be observed in nature, shewing how with immense variety in detail there is little in principle. With regard to the water vascular system, he thought there was no reason why the organs should not be respiratory as well as excretory; these functions being not unfrequently blended in the lower animals.

Mr SEELEY did not admit Prof. M'Coy's claim to the relation of the muscles and valves; denied that the palpæ in the Lamellibranchs corresponded in function with the spiral coils of the Brachiopods. These and the external covering of the brachiopod correspond much more nearly with those of the polyzoa; reference, he held, should rather be made to the radiata than the lamellibranchs. As a case in point, he instanced perforations common in the brachiopod shells. He thought the brachiopod diverged almost as far from the ordinary molluscan type, as do the echinodermata from the orders to which they are most nearly related.

Mr SALTER in combating Mr Seeley's remarks, said he preferred to seek analogies in those orders which had most resemblance rather than in those which had no external resemblance.

February 22, 1869.

The PRESIDENT (PROFESSOR SELWYN, D.D.), in the Chair.

Communications to the Society:

(1) *On the bird-like characters of the Brain and Metatarsus in the Pterodactylus from the Cambridge Greensand. By* Mr H. SEELEY, F.G.S.

This was a note upon a specimen of the skull of Pterodactylus, which had been obtained by Mr Walker (Sidney Sussex College), and placed by him in Mr Seeley's hands. The latter,

on examining it, found that the cast of the brain was remarkably clear and perfect. The characteristics disclosed by this cast, when developed, were very remarkable; it did not resemble those of the pisces or reptilia, but presented affinities to some extent with the birds, and still more with the lowest orders of mammalia, especially with the ornithorhynchus. This discovery corroborated the theory advanced some time since by Mr Seeley, that the Pterodactyles were much more nearly allied to the birds than to the reptiles.

(2) *Note on the Pterodactylus macrurus (Seeley) a new species from the Purbeck Limestone, indicated by caudal vertebræ five inches long. By* Mr H. Seeley, F.G.S.

This specimen was remarkable for its extraordinary size, the largest that has been obtained from the Cambridge chloritic marl being one and a half inches long.

(3) *Note on the thinning away to the westward in the Isle of Purbeck of the Wealden and Lower Greensand strata. By* Mr H. Seeley, F.G.S.

The various sections made by Mr Seeley, in company with Mr Sedley Taylor, of Trinity College, were carefully described and tabulated.

(4) *On the coincidence of the Moon's periods of rotation on her axis and synodical revolution round the earth as an electro-magnetic phenomenon. By* Mr Potter.

The whole discussion of the moon's rotation on her axis commencing with Newton's original solution, supposes that the periods of rotation on her axis and of her synodical revolution round the earth, were *equal* or *nearly equal*, whilst her mass

was in the fluid state, and that her figure became ellipsoidal with the longer axis towards the earth under the actions of the centrifugal force from rotation, and the ablatitious force of the earth's attraction on the fluid mass.

The librations of the moon commencing with the diurnal libration discovered by Galileo, and the libration in longitude discovered by Hevelius, as well as the libration in latitude were explained by Newton on the above supposition. Cassini, the celebrated French Astronomer Royal, discovered the remarkable property of the lunar motions that the nodes of her equator coincide with the nodes of her orbit, and that a plane through her centre parallel to the plane of the ecliptic lies between the planes of her equator and her orbit, so that the poles of the ecliptic, her orbit, and her equator are in the same great circle, but the two latter on opposite sides of the first. He concluded that the inclination of the lunar equator to the ecliptic was 2° 30′.

Mayer, in the middle of the last century, found the inclination of the moon's equator to the ecliptic to be 1° 45′. Lalande later found it to be 2° 9′, but recent observations confirm Mayer's result.

These are called apparent librations, but Newton discussed the existence of a real libration or oscillation of the longer axis of the moon's figure about its mean place. D'Alembert, Lagrange, and Laplace applied refined analytical methods to this problem, but the conclusion of Professor Grant of Glasgow, Fellow of the Royal Astronomical Society, in a note in his excellent work, the History of Astronomy, has the following: "It is natural enough, indeed, to suppose that the illustrious author of the Principia did not feel any anxiety to repudiate the original equality of the motions of rotation and revolution —a relation which, although perhaps difficult to explain by the doctrine of chances, becomes very interesting and suggestive when it is considered as the result of Supreme Intelligence."

The whole of the discussions of these eminent men involved this supposition of the *equality* or *near equality* of the periods of rotation and revolution whilst the moon's figure was forming, as the bases of their solutions, and having only the theory of gravitation as the foundation of their methods, they had no option left to them.

Since about the year 1820, we have gradually become acquainted with other powerful forces in nature. Œrsted's discovery that a magnetic needle sets itself at right angles to a circuit over or under which it is placed when a galvanic current passes along that circuit, was quickly followed by others still more surprising, and the science of electro-magnetism in its various branches, has in less than 50 years from its birth grown to be a most extensive as well as a most important science. The branch which we call magneto-electricity arose afterwards, and it is to the forces of magneto-electricity that I shall refer for explanation of the lunar phenomena before related.

I must be allowed to diverge from the simple course of procedure to remind the meeting that bodies may be now arranged in two classes which we call magnetic and diamagnetic bodies. The class of magnetic bodies comprehends those which are attracted towards the poles of powerful magnets, and the class of diamagnetic bodies comprehends all the others, whether neutral or repelled from the poles of powerful magnets. The first class comprehends many metals, as iron, nickel, cobalt, manganese, chromium, &c., crown-glass, potassium, sodium, many salts and oxides of the magnetic metals, as well as magnetic minerals, and oxygen gas. The second class contains bismuth, phosphorus, antimony, heavy glass, or silicated borate of lead, zinc, &c., flint-glass, mercury, lead, silver, copper, gold, &c., nitrogen gas.

Hot air is diamagnetic with respect to cold air, and hence the peculiar action of powerful magnets on flames.

The repulsion of the most diamagnetic substances, as bis-

muth, phosphorus, &c. from the poles of magnets is a very weak force compared with the attraction of the more magnetic metals. We recognize also that magnetic bodies have the property of *polarity*, which does not exist in diamagnetic bodies.

The discovery of Dr Faraday that oxygen gas is magnetic was a most important step, and we may now conclude that the earth's magnetism resides very greatly, if not entirely, in the oxygen gas of the atmosphere, for the greater part of the earthy materials of the earth's crust are diamagnetic substances, and the abundant mineral per-oxide of iron is still small in quantity compared with the others, and only feebly magnetic.

It is not easy to find the place of oxygen gas amongst the magnetic substances from the small weight contained in such glass globes as we can use in experiments, but the black or protoxide of iron is very magnetic. In the original experiment exhibited by Dr Faraday, when he made known his discovery at the meeting of the Royal Institution, at which I was present, the weight of the oxygen gas in the globe of perhaps 2 inches diameter would be less than 1½ grains, and the flint-glass of the globe was feebly diamagnetic yet though surrounded by atmospheric air of which about $\frac{1}{5}^{th}$ of the volume is oxygen gas; and notwithstanding the resistance to motion which such a globe would experience, and its inertia, yet it was evidently steadily though slowly attracted to the poles of the magnet. Since that time, I, every session of my lectures, exhibited the same result without a single case of failure, to my experimental class of Natural Philosophy in University College, London.

If we consider that the weight of the oxygen gas of the atmosphere is more than $\frac{1}{5}^{th}$ of the weight of the barometric column or more than the weight of 6 inches depth of mercury covering the whole surface of the earth or considerably more than that of 10 inches depth of iron, we may conclude that the earth's magnetism lies very greatly if not entirely in the

oxygen gas of the atmosphere, of which each particle is a small magnet having its north and south pole. Now substances which are magnetic lose this property at high temperatures, and the late Professor Barlow, of Woolwich, in his treatise on magnetism, states that *white hot* iron ceases altogether to be magnetic. So that, if the magnetic metals exist in large quantities in the interior of the earth, they will cease to be magnetic, from the high temperature of the internal heat. From these considerations, it is probable that the earth owes its claim to be a magnetic body to the oxygen of the atmosphere, and the coldest parts on the earth's surface are the localities of the magnetic poles, the definition of the magnetic pole being the place where the dipping needle takes the vertical position; the north magnetic pole, in round numbers, being in 70° North latitude, and 100° West longitude.

According to Professor Barlow and Mr Charles Bonnycastle, the investigation of the magnetic meridians in various places shews that they only converge ultimately towards some places within the arctic and antarctic circles.

The moon having no visible atmosphere we have no like reasons to offer for her being a magnetic body, and the volcanic appearance of her surface has all the appearance of the diamagnetic substances, and the interior still fluid, as shewn by the occasional volcanic eruptions which are noticed, will also from the great heat be diamagnetic.

The relations of the earth and moon are therefore those of a smaller diamagnetic body in the presence of a larger magnetic body, and the phenomena of magneto-electricity will be effective between them, since the magnetic force acts freely through a vacuum as well as through dense bodies.

About the year 1825, M. Arago made the discovery that if a circular disk of copper be set rotating rapidly parallel to and under a bar magnet which is suspended so as to be free to move parallel to the disc, then the magnet goes into rapid rotation

in the same direction as the disc. Sir John Herschel and Mr Babbage[1] soon afterwards tried the converse experiment of causing the magnet to rotate parallel to a suspended disc, when the disc commenced to rotate in the same direction as the magnet. Now in both these cases if the suspended body were prevented from rotating, a reaction tending to destroy its rotation would take place upon the rotating body. The science of magneto-electricity being unknown at the time of these discoveries, they were inexplicable from previously known properties of magnetic and non-magnetic bodies. We now know[2] that when a diamagnetic body is set in motion near a magnet, that electrical currents arise in the moving body and by Œrsted's discovery, the magnet tends to set itself at right angles to the current thus formed, and so goes in Arago's experiment into rotation; and if prevented doing so, it reacts on the rotating body to destroy its rotation.

The converse experiment to the above is a very impressive one and more directly applying to our present subject. It is the immediate and sudden destruction of rotatory motion in an angular diamagnetic body rotating near the poles of a powerful electro-magnet, when the current is formed. The sudden destruction of the momentum in a heavy rotating square prism of copper in such a case surprises us when first seen, from there being no evident force to produce it.

Now in the case of the magnetic earth and diamagnetic moon, the same forces but in different degrees must arise, and if the moon ever possessed or received a rotatory motion, *relative to the earth*, it would gradually but surely cease.

The moon's absolute rotatory motion in her synodic period forms a no rotatory motion with respect to the earth; which is the point involved in the magneto-electric phenomena. Some years ago there was an active controversy as to whether

[1] See their paper read before the Royal Society, 16th June, 1825.

[2] See Faraday's paper read before the Royal Society, 24th Nov. 1831.

the moon had really any rotatory motion, but the controversy ceased when attention was called to the necessary distinctions between relative and absolute motions.

As the same law of no rotatory motion of the satellites of Jupiter with respect to their primary is believed to exist, we are led to conclude that Jupiter is a magnetic body, and his satellites are diamagnetic bodies.

March 8, 1869.

The PRESIDENT (PROFESSOR SELWYN, D.D.) in the Chair.

The Meeting was held, by permission of the Museums and Lecture Rooms Syndicate, in the Anatomical Lecture Room, New Museums.

The following new Fellows were elected:

Rev. G. HENSLOW, M.A., *Christ's.*
RICHARD C. JEBB, M.A., *Trinity.*
LINNÆUS CUMMING, M.A., *Trinity.*

On the generation of clouds by actinic action, and the reaction of such clouds upon light. By Professor TYNDALL, F.R.S.

Professor Tyndall commenced by referring to the distinction drawn by Fichte between the processes used by two different classes of minds to arrive at clearness of religious belief, the logical and intuitive methods. These methods, he said, had a no less important bearing upon the observation of external nature. To *know* nature's operations required a long and careful scientific training, but to *see* them was in the power of men of culture and imagination. Perhaps an image of natural processes placed before even persons destitute of the scientific training would be interesting, and might woo some of those who had hitherto been *seers* to be *knowers* of nature.

He then proceeded to perform an experiment shewing new actions effected under new conditions, rays of light producing chemical action. Into a long glass tube carefully exhausted, a small quantity of air had been allowed to pass through tubes containing (1) cotton wool, (2) caustic potash and marble, (3) sulphuric acid and glass, (4) nitrite of amyl, with the vapour of which last substance the air in the experimenting tube was loaded.

A beam of electric light was then passed along the tube, and a beautiful cloud was at once formed, gradually extending from the end nearest the lamp towards the other. The tube was then reversed, and the same phenomenon produced at the other end. What, asked the lecturer, is the vapour in which this change has been produced? A collection of molecules, each molecule being built up of nineteen atoms; the waves of light beating against these invisible molecules have broken them up and re-arranged them in such a form that they become visible.

In this case the vapour of the substance itself was directly exposed to the action of the light; other substances require to be introduced into the experimenting tube in connexion with vapours which aid as it were the process of decomposition. Benzole vapour for example is unaffected, but if mixed with air passed through aqueous nitric acid, with which it has a *tendency* to combine, it is precipitated at once on the beam of light. This is an illustration of one of the commonest of nature's operations, vegetation: the carbonic acid and the chlorophyll are side by side in leaves, ready to unite but unable to do so of themselves; the ray of sunshine falling on the leaves consummates the union, gives the green colour to the leaves, and sets oxygen free into the air.

The action of which an example has been given might be made very slow if the vapour tested were sufficiently attenuated; the growth of the visible particles might be very slow. What should we expect under these circumstances?

The lecturer here paused to exhibit Newton's experiment of the decomposition of a beam of white light into its constituent coloured rays, and referring to a diagram shewed that the energy of the waves which produced the sensation of red was many times as great as that of those which produced blue. Then naturally we should expect that the blue vibrations (using this term for the sake of brevity), having least energy, would be soonest stopped by particles floating in the medium through which the light passed. That this was so in fact the next experiment clearly shewed: the cloud produced in the vapour of nitrite of butyl (though not so clearly produced as on some previous occasions), was distinctly visible to those who were near the table, and was of a blue or violet shade. The light emitted from the cloud was moreover perfectly polarized, as was tested by the interposition between it and the eye of a crystal of tourmaline. The next experiment, owing to some defect in the apparatus, the preparation of which had been necessarily hurried, did not succeed, but was described by the Professor as the excitation of a real cloud within the tube containing an actinic cloud and the reproduction on a small scale of the azure of the sky. It was, as he described it, taking a piece of the sky and producing in limited space all the phenomena of cloud light and polarization which are produced by the sun light and the clouds of heaven.

To shew the exceeding minuteness of the particles to which the blue colour of the sky is due, Professor Tyndall described the difficulty of getting rid of the residue of vapour in the experimenting tube, a difficulty which had more than once led him to false conclusions until the experiment which the residuary vapour vitiated had been repeated. The last experiment was an examination of the vapour by means of polarized light, in which the cloud, distinctly visible in one direction, was entirely lost to sight when the polarizing prism had been revolved through an angle of forty degrees.

But the most interesting part of the lecture was the conclusion; the former parts were not absolutely novel, but the latter had never been announced before. These clouds and the similarity of their appearance to the tails of comets, have led Prof. Tyndall to form a Theory of Comets, which he enunciated on this occasion for the first time. The theory, which at any rate collects and accounts for observed facts, is as follows:—

1. The theory is that a comet is composed of vapour decomposable by solar light, the visible head and tail being an actinic cloud resulting from such decomposition. The texture of an actinic cloud is exactly that of a comet.

2. The tail, according to this theory, is not projected matter, but matter precipitated on the solar beams traversing the cometary atmosphere. It can be proved by experiment, that this precipitation may occur with comparative slowness along the beam, or that its consummation may be practically momentary throughout the whole length of the beam. The amazing rapidity of the development of the tail would be thus accounted for without invoking any motion of translation save that of the solar beams.

3. As the comet wheels round its perihelion, the tail is not composed throughout of the same matter, but of new matter precipitated on the solar beams which cross the cometary atmosphere in new directions. The enormous whirling of the tail is thus accounted for without invoking a motion of translation.

4. The tail is always turned from the sun for this reason. Two antagonistic powers are brought to bear upon the cometary vapour: the one an actinic power tending to produce precipitation; the other a calorific power tending to effect vaporization. Where the former prevails we have the cometary cloud; where the latter prevails we have transparent cometary vapour. As a matter of fact, the sun emits the two powers whose agency is here invoked. There is nothing hypothetical in the assumption

of their existence. That precipitation should occur behind the head of the comet or in the space occupied by the head's shadow, it is only necessary to assume that the sun's calorific rays are absorbed more copiously by the head and nucleus than the actinic rays. This augments the relative superiority of the actinic rays behind the head, and enables them to bring down the cloud which constitutes the comet's tail.

5. The old tail, as it ceases to be screened by the nucleus, is dissipated by the solar heat; but its dissipation is not instantaneous. The tail leans toward that portion of space last quitted by the comet, a fact of observation being thus accounted for.

6. In the struggle for mastery of the two classes of rays, a temporary advantage, owing to variations of density or some other causes, may be gained by the actinic rays even in parts of the cometary atmosphere which are unscreened by the nucleus. Occasional lateral streamers and the apparent emission of feeble tails towards the sun would be thus accounted for.

7. The shrinking of the head in the vicinity of the sun is caused by the beating against it of the calorific waves, which dissipate its attenuated fringe, and cause its apparent contraction.

At the conclusion of the lecture, which was listened to with the greatest attention by a large audience, the President (Rev. Professor Selwyn) conveyed the thanks of the Society to the lecturer, who briefly acknowledged the compliment, and expressed his pleasure in being able to appear before the Society.

NOTICE.

Vol. XI. Part II. of the Transactions of the Society is ready, and every Fellow of the Society who has paid his subscription for the current year may have one copy gratis upon sending a written application to Professor LIVEING, Secretary.

Fellows requiring the Part to be sent to them must transmit postage stamps to the value of 6*d*. The copies of those who do not send stamps will be left at the Porter's Lodge at the New Museums; but no copy will be issued until the Meeting of the Society next after the receipt of the application for the same.

(PART XI.)

PROCEEDINGS

OF THE

Cambridge Philosophical Society.

[*Reprinted, April* 1898.]

12

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Cambridge:
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AT THE UNIVERSITY PRESS.

April 12, 1869.

The PRESIDENT (PROFESSOR SELWYN, D.D.) in the Chair.

The PRESIDENT presented a set of photographs of the planet Mercury, during its transit over the Sun, Nov. 5th, 1868, taken at Ely, which were of unusual interest as being the only photographs taken in England on that occasion.

The thanks of the Society were returned to him, and also to Professor CHALLIS for a copy of his work on *Principles of Mathematics and Physics*.

The Treasurer (Mr CAMPION) presented his accounts, and congratulated the Society on its improved and improving financial condition.—The thanks of the Society, proposed by Dr PAGET and seconded by the MASTER OF TRINITY, were returned to the Treasurer.

Communications made to the Society:

A new interpretation of a disputed passage in Thucydides IV. 30. *By* Professor KENNEDY, D.D.

The main purpose of this paper was to shew that the word *αὐτοῦ* before *ἐσπέμπειν* was to be taken as a genitive pronoun and referred to *τὸν σῖτον*, meaning that the General was sending in a supply of corn for a number smaller than itself, viz. than was represented by the quantity of corn (which was delivered in rations for each man).

Mr PALEY, while admitting the value of the paper, doubted whether the collocation of *αὐτοῦ* after instead of before *τὸν σῖτον* quite suited Professor Kennedy's interpretation.

Mr MUNRO agreed with Professor Kennedy; saying that *αὐτοῦ* in the sense of "there" appeared to him quite otiose and unmeaning, and thought that the collocation did not present any difficulty.

Mr W. C. GREEN remarked upon a difficulty in the use of ποιεῖσθαι, and rather agreed with Mr Paley about the position of αὐτοῦ.

Mr FENNELL defended the reading αὐτούς, thinking it to mean that the Lacedæmonians were trying to make their number appear smaller than it was, in order that the Athenians might think little of their advantage.

Mr HAMMOND saw no difficulty so far as the collocation of αὐτοῦ went, but rather preferred the reading αὐτοῖς; and shewed reasons why the Spartans would be very anxious to make peace at that period of the war; and expressed an opinion that the provisions introduced had been such as would not keep.

Some conversation also took place, in which the above, the MASTER OF TRINITY, the PROVOST OF KING'S, and the PRESIDENT took part.

The PRESIDENT also mentioned that, if he remembered right, Bauer had hinted at this meaning of αὐτοῦ, and thought the Professor's translation of αὐτοῦ rather harsh.

April 26, 1869.

Communications made to the Society:

(1) *Note on passages in Aristotle's Meteorologica and in Sir J. Herschel's Astronomy relating to the sight of faint objects, and on some passages of Ancient Poets relating to the lost Pleiad. By* the President, Professor SELWYN, D.D.

The fact that there seemed to be, as noticed by the above authorities, a lesser sensibility of that part of the retina in the line of direct vision, the author thought might be due to the greater use of that part in the ordinary work of the eye. He

then passed on to make some remarks on the Pleiads mentioned by the Greek Poets sometimes as six, sometimes as seven, and concluded that the uncertainty of the number was due to the above fact.

Professor KENNEDY made some remarks on the derivations of the name Pleiad.

Professor CHALLIS thought that one of the stars might be a variable one.

Mr POTTER made a few remarks on the appearances presented by the blood vessels in the eye.

Professor MILLER said these could well be seen on waking suddenly.

Mr TROTTER mentioned that, as shewn by some experiments lately made at Heidelberg, the focus centralis took a longer time to receive the impression of objects, but retained it longer.

Dr PAGET thought the matter required further experiments, and mentioned ways in which it would be desirable to test it.

Some further conversation occurred, in which the MASTER OF TRINITY, Mr W. C. GREEN, and the above took part.

(2) *On new and general Equations for the Equilibrium of Flexible Surfaces.* *By* RICHARD POTTER, A.M.

[*Abstract.*]

In a paper on the "curvæ lintearia" of John Bernoulli which the author read in May 1868 before the Society, he stated that he believed the equations which Poisson had investigated in his "Mémoire sur les surfaces élastiques" read before the French Institute on the 1st August 1814, might be brought to comprehend all the ordinary cases to which we wish to apply them, by restoring a factor which M. Poisson had struck out. Soon after reading the paper he came to a different conclusion, and undertook to investigate the equations for the equilibrium of flexible surfaces, from mechanical rather than mathematical

considerations, and found the problem to be much simpler than had been generally supposed, and that the three necessary and sufficient equations for the equilibrium of an element of the surface could be obtained in a very simple manner.

By the principles of statical science, when three forces are in equilibrium at a point they must be in the same plane, because the resultant of any two must be equal in magnitude and opposite in direction to the third force. We know therefore that when two opposing tensions and an external force are in equilibrium at an elementary *area* of a flexible surface, the same rule must hold good; and it also must hold good if the tensions transmitted in any manner through a sheet are equivalent to resultant tensions acting in different directions through the element of the surface, for each set of resultant tensions and their corresponding portions of the external forces. From these considerations we learn, that in all the ordinary problems of the equilibrium of flexible surfaces, of regular forms and symmetrical positions, where the external forces arise from gravity or the pressure of fluids, the tensions will act along the lines of curvature of the sheet, since it is only for points taken in succession along such lines, at right angles to each other at each point, that consecutive normals to the surface meet, and the conditions can be satisfied.

If s and s' are arcs of the lines along which the resultant tensions act, measured from any fixed points; ds and ds' elements of these arcs at right angles to each other, forming the sides of an elementary area $ds \, . \, ds'$ upon the sheet, of which the thickness at this element is τ, and density ρ; then $\rho\tau ds \, . \, ds'$ is the mass of the element. Let X, Y, Z be the external accelerating forces, acting parallel to the axes of coordinates respectively upon the elementary mass. Let T be the tension due to a unit of breadth, acting in the direction of s upon the element, T' that acting in the direction of s'; with dx, dy, dz the components of ds in the axes respectively; and dx', dy', dz' those of ds'.

Then the component tensions acting on the element in the directions of the axes respectively, will be

$$Tds' \cdot \frac{dx}{ds}, \qquad Tds' \cdot \frac{dy}{ds}, \qquad Tds' \cdot \frac{dz}{ds}$$

for Tds' the tension acting on the element in the direction of s; and

$$T'ds \cdot \frac{dx'}{ds'}, \qquad T'ds \cdot \frac{dy'}{ds'}, \qquad T'ds \cdot \frac{dz'}{ds'}$$

those for $T'ds$.

Now the variations of these in passing from one side of the element to the opposite side will be the only internal forces entering the equations of equilibrium; and the three necessary and sufficient equations for the equilibrium of the element under the action of the external and internal forces become as follows,

$$X\tau\rho \cdot ds \cdot ds' + \frac{d\left(Tds' \cdot \frac{dx}{ds}\right)}{ds} \cdot ds + \frac{d\left(T'ds \cdot \frac{dx'}{ds'}\right)}{ds'} \cdot ds' = 0 \ldots\ldots (1),$$

$$Y\tau\rho \cdot ds \cdot ds' + \frac{d\left(Tds' \cdot \frac{dy}{ds}\right)}{ds} \cdot ds + \frac{d\left(T'ds \cdot \frac{dy'}{ds'}\right)}{ds'} \cdot ds' = 0 \ldots\ldots (2),$$

$$Z\tau\rho \cdot ds \cdot ds' + \frac{d\left(Tds' \cdot \frac{dz}{ds}\right)}{ds} \cdot ds + \frac{d\left(T'ds \cdot \frac{dz'}{ds'}\right)}{ds'} \cdot ds' = 0 \ldots\ldots (3),$$

which are applicable generally to the cases of the equilibrium of flexible surfaces.

When the external force is a normal pressure N on a unit of area, such as the pressure of a fluid, and R, R' are the principal radii of curvature at the element, then by resolving in the direction of the normal and performing the differentiations, we obtain the well known formula $N = \frac{T}{R} + \frac{T'}{R'}$ whether T and T' be constant or variable since the coefficients of $\frac{dT}{ds}$ and $\frac{dT'}{ds'}$ disappear.

The case of the catenary curve for a heavy rectangular sheet suspended by two of its opposite sides from two parallel horizontal straight lines is easily and concisely discussed from these equations.

The case of the "curva velaria" of James Bernoulli is easily investigated by means of them.

The case of the form which a piece of bladder tied over the circular aperture in the receiver of an air-pump, when a portion of air is withdrawn from the interior, depends on the difference of the pressures on the internal and external surfaces, and the nature of the surface to bear them. When the form the bladder takes is known then the tensions at given points are found from the formulæ.

The phenomena of capillary attraction being those of flexible surfaces, the results which the author had obtained by longer methods of procedure are more concisely investigated by means of these equations.

May 10, 1869.

The President (Professor SELWYN, D.D.) in the Chair.

New Fellows elected:

RICHARD SHILLETO, M.A., *St Peter's College.*

THOMAS M'KENNY HUGHES, M.A., *Trinity College.*

Communications made to the Society:

(1) *On a Group of Figures with archaic inscriptions on one of the Leake Vases in the Fitzwilliam Museum. By* MR PALEY.

This vase has been described as "the invasion of Troy by Hercules." Mr Paley thought it perhaps rather the conflict of Hercules with the Amazons. The interest of it was that it bore in some respect on the Homeric question; for it alluded to a legend mentioned by Pindar but not by the Homer whose works we now possess. Mr Paley first spoke of the purpose of vases in tombs, doubtless to contain food for the use of

the ghost; he then described a fragment of a lecythus from Smyrna—after which he quoted the allusions to the Legend of Hercules and his attack on Troy from Pindar. This invasion is here represented in the vase, names being given to the figures—Andromache being one of them. The Leake vase came from Vulci, Etruria: it is an amphora about 18 inches high, belonging rather to the archaic than the fine art period: it represents Hercules and Telamon fighting against six women, armed with circular shields and broad bladed spears; the shield bearing devices: character of ornamentation, Assyrian. Six of the nine names are written backwards; E, O, are used for H, Ω; H is used as rough breathing; the forms of other letters are peculiar. He also entered at length into other points in the Legend.

(2) *Is volition a function of material forces only? and can a Planet exist as a habitable world for ever? By* Mr RÖHRS.

The argument in this paper was of too abstruse a nature to admit of abstracting.

Mr MOULTON and Mr CLIFFORD made some remarks objecting to the theory advanced in the paper.

(3) *A series of comparative views of the Solar Disc, and of Planetary Configurations. By the* President, Professor SELWYN, D.D.

Professor SELWYN briefly described the mode of taking heliographs of the sun, carried on during five years at Ely. On the back of each photograph the planetary configuration was inscribed, with a view to ascertain the connexion between the planet and solar spots.

Professor CHALLIS thought the investigation important, and mentioned one or two well-known facts concerning the solar spots.

October 25, 1869 (Annual General Meeting).

Professor CAYLEY in the Chair.

The following were elected Officers of the Society:

President.

Professor CAYLEY.

Vice-Presidents.

Professor SELWYN.
Rev. W. G. CLARK.
Mr TODHUNTER.

Treasurer.

Rev. W. M. CAMPION.

Secretaries.

Professor C. C. BABINGTON.
Professor LIVEING.
Rev. T. G. BONNEY.

New Members of the Council.

THE MASTER OF GONVILLE AND CAIUS.
Dr P. W. LATHAM.
Rev. J. C. W. ELLIS.
Mr P. T. MAIN.

Communications made to the Society:

(1) *On some supposed Pholas Burrows in Carboniferous Limestone Rocks. By* T. G. BONNEY, B.D.

The following are the results of the author's observations on burrows on the Great and Little Orme's Head:—(1) They are clearly the result of the action, mechanical, chemical, or

both, of some living agent. Many of them are in positions where rain or wind cannot reach them, run almost vertically up into the rock, and are practically impervious to water at the highest point. (2) They are rarest on surfaces much exposed to prevailing winds, or where the rock approaches to a grit. (3) They usually occur on boulders, or projecting rocks, at no great distance from the surface of the soil; in not a few cases the turf had actually grown into them. (4) The axis of the burrow usually is not at right angles to the surface of the rock; often is only inclined at a slight angle to it, so that the burrow commences as a channel (if this be not a natural depression utilised), and sinks gradually into the rock. Frequently it is driven into some slight prominence, as though the burrowing animal had first sheltered itself under the lee of this, and then gradually worked its way deeper into the rock. (5) The burrows are very frequently curved. Sometimes the tangents to the axis at the two extremities, if produced to meet, would include an angle not very much greater than 90°. (6) *Helices*, especially *H. adspersa*, are generally abundant in the neighbourhood of these burrows; empty shells are common in them, and in the freshest, smoothest, and most unweathered of them, he always found a living *Helix*. (7) The constriction in the upper part of the burrow characteristic of perfect Pholas excavations, is generally wanting, and though the burrow sometimes contracts towards the mouth, this is often not quite regular in form; so that a *Helix* that would exactly fit the end would be able to quit the burrow. At least he believes this to have been the case with all that he examined. The ends, also, of the burrows are, he thinks, generally rather flatter than is usual with Pholas holes.

From the above considerations, and especially from the position of some cavities in the roof of a narrow crack in the rock, the conclusion is, he thinks, irresistible, that these are not the

weathered burrows of departed *Pholades*, but have been and are being hollowed out by *Helices*, the principal, if not the only agent, being *H. adspersa*. A specimen of the burrows was exhibited.

Mr SEELEY described *Helices* which he had seen at Charlton (Kent), which hollowed themselves small depressions in cracks in the chalk. Also Pholas burrows which he had seen in soft limestone in various parts of the south of England. These were very unlike the specimen handed round. He thought, however, that although Mr Bonney had admitted it, the burrows need not have been effaced by submersion, giving instances of their preservation.

Mr BONNEY replied that he thought that the Carboniferous limestone if immersed must have been weathered, and pointed out that most of the instances adduced by Mr Seeley were in harder rocks.

Mr FISHER said that the holes not being vertical to the surface was fatal to their being Pholades, but quoted the opinion of Mr Pengelly, who thought they were made by some marine mollusks.

(2) *Tidal phenomena investigated according to the laws of fluid motion, taking into account fluid friction. By* Mr RÖHRS.

Professor ADAMS made some remarks on the difficulty of the subject.

The ASTRONOMER ROYAL made a remark upon Laplace's theory of the tide.

Professor CHALLIS stated the mode in which he should wish the problem attempted.

(3) Mr FISHER exhibited a flint implement which he had found on a heap of gravel which was stated to have been dug at Chesterton.

November 8, 1869.

Professor CAYLEY, and during part of the evening Professor CHALLIS, in the Chair.

New Fellow elected:

Rev. E. K. GREEN, M.A., *St John's College.*

Communications made to the Society:

(1) *On a certain Sextic Torse. By* Professor CAYLEY.

(2) *The Bedawin of Sinai and their traditions. By* Mr E. H. PALMER.

The Arabs were much less wanderers than was generally supposed in Europe, seldom moving except from winter to summer camps; though their ideas of right of property differed from our own, their character was far better than usually believed. They have no history because no *nationality*, only a *clanship*; therefore there is rarely any concerted action. There are about 4000 grown males in the Towarah or Sinai Bedawin. They are not aboriginal, but came with the Mohammedan conquerors; the aboriginals were an Aramæan race, to be found perhaps among the Jebaliyeh or mountain tribe. Mr Palmer then mentioned the various divisions of the tribes. The Sheikh was rather an arbitrator than an adjudicator, negotiating business for the tribe; he, however, interposed to make equitable arrangement in cases of debt or theft, which latter is rare. The Agyd is a military officer, hereditary and only bearing office in time of war. The mode of marriage was then described. The bridegroom calls on the father, and a price is arranged, after which great rejoicings take place; there is then a ceremony of betrothal by pressing a piece of herb wrapped in a turban before the Khatib (the bride is not consulted). On her return the bridegroom's mantle is suddenly

thrown over her by the Khatib who pronounces the bridegroom's name. Various ceremonies are gone through for three days, and then she is taken home to her husband's house. Death— The corpse is washed, a little bag of corn placed beside it, and it is then lowered into the grave; certain prayers being said over it, the grave is filled, and a feast is held. The women bewail the dead with loud cries.

The Arabs are not an irreligious race, though less demonstrative than other eastern races in their observances. Their prayers were described by Mr Palmer, who repeated one of them. They believe that the monks of the Convent can bring rain. They believe in a general resurrection, when the world shall melt; the good will rise with their hands above their heads, the wicked with their hands by their sides; vultures come, the former can drive them away, but the latter have their eyes pecked out. They believe that snakes may be seen fighting for a stone, which, if secured, gives immunity from snake bites. Several curious superstitions about the Convent were related. Arab tradition, though influenced by monkish legend, contains independent evidence of the Exodus. This is the legend of the departure from Egypt: Moses and Pharaoh having quarrelled, the former fled with the Israelites; the Egyptians followed and were drowned much as described in the Bible, save that the Hammam Pharown was supposed to be formed by Pharaoh's drowning struggles. The name of Moses was attached to many spots in the peninsula. Among others, the Arabs point out a rock as struck by Moses, and severed by his sword because it impeded his path. The rock at Rephedim is an invention of the monks, but in Wady Feiran they shew a rock from which they say Moses obtained water, when the Israelites were athirst. Another rock is said to have had water drawn from it. The primitive dwellings are called Mosquito houses, said to have been raised to protect the Israelites from a plague of mosquitoes. They point

out some ruins near Hazeroth as belonging to a caravan who afterwards were lost in the Tîh, and never heard of again. There was other corroborative evidence to shew that this last legend referred to the Israelites.

November 22, 1869.

The President (Professor CAYLEY) in the Chair.

New Fellow elected:

S. S. LEWIS, B.A., *Corpus Christi College.*

Communications made to the Society:

(1) *On the Degeneration of Curves. By* W. K. CLIFFORD, M.A., *Trinity College.*

(2) *On a Machine for solving Equations. By* Mr J. C. W. ELLIS.

(3) *The Reptiles of the Kimeridge Clay of Cambridgeshire. By* Mr H. SEELEY, F.G.S.

To which was added a note on an animal of the Pterodactyle kind from the Wealden, which was larger than any known land animal.

February 21, 1870.

The President (Professor CAYLEY) in the Chair.

Communications made to the Society:—

(1) *On the Antiquity of some of our familiar Agricultural Terms. By* F. A. PALEY, M.A.

The author pointed out that the digamma sound was still retained in English, as for example in our numeral "one;" so that this form was more ancient than that of εἷς, which was once digammated. He then called attention to the tendency of language to reveal primitive forms, and to the fact that agricultural life was favourable to the preservation of old words. Probably many old Aryan words still survive among the peasantry.

Niebuhr observed that agricultural terms were generally of common origin in both Greek and Latin, though the Oscan war terms were without representatives in Greek. In English it would be noticed that while the generic names of animals were usually of Saxon origin, the words denoting their application were of Latin or Greek derivation; thus the words for cooked flesh were from the Norman. Words when not generic, but particular and descriptive, generally appear to have representatives in the classical languages. Mr Paley then gave a large number of instances of these rules, concluding with some remarks on the antiquity of "plough" and "harrow." The former, he thought, was connected with the root of πλέω, and he noticed the frequent metaphorical use in poetry, as of a ship "ploughing the water." "Harrow" he connected with the root of χαράσσω, and the word "harass."

Professor SELWYN asked whether *to ear* was still used in England for *to plough*, as in Chaucer, Shakespeare, and the Bible?

Mr LUMBY objected to ἄχυρον being referred to the same root as "chaff," as Mr Paley had done. He agreed with him in rejecting the popular derivation of "gallop," and mentioned a confirmation of the derivation of the word "bull" (*bubulus*) in "bugle," which is used by Sir John Maundeville for "bull;" afterwards for a musical instrument made from the horns.

Mr W. C. GREEN thought "plough" might be from the same root as πλήσσω.

(2) *Proof that every Rational Equation has a Root. By* W. K. CLIFFORD, B.A., *Trinity College.*

[*Abstract.*]

The proof contained in the present communication depends on the determination of a quadratic factor of the rational integral expression

$$x^{2s} + a_1 x^{2s-1} + a_2 x^{2s-2} + \ldots + a_{2s}.$$

On dividing this expression by $x^2+p_1x+p_2$, we obtain by the ordinary algebraic rules a remainder of the form $M_{2s-1}x+N_{2s}$, where M_{2s-1} and N_{2s} are functions of p_1 and p_2 whose weights are $2s-1$ and $2s$ respectively, and which may accordingly be written in the forms

$$M_{2s-1}=b_{2s-1}+p_2b_{2s-3}+\ldots\ldots+p_2^{s-1}b_1,$$
$$N_{2s}=c_{2s}\quad+p_2c_{2s-2}+\ldots\ldots+p_2^{s},$$

where the b, c are of an order in p_1 indicated by their suffixes. On writing down (by Professor Sylvester's Dialytic method) the result of eliminating p_2 between these equations, it is at once apparent that this resultant is of the order $s(2s-1)$. Thus the determination of a quadratic factor of an expression of degree $2s$ is reduced to the solution of an equation of order $s(2s-1)$. But this number is *one degree more odd* than the original number $2s$; that is to say, if the number $2s$ is 2^k multiplied by an odd number, then $s(2s-1)$ is 2^{k-1} multiplied by an odd number. Hence by a repetition of this process we shall ultimately arrive at an equation of odd order, which, as is well known, must have a real root. By then retracing our steps the existence of a quadratic factor of the original expression is demonstrated.

(3) *On the Space-Theory of Matter.* By W. K. CLIFFORD, B.A., *Trinity College.*

[*Abstract.*]

RIEMANN has shewn that as there are different kinds of lines and surfaces, so there are different kinds of space of three dimensions; and that we can only find out by experience to which of these kinds the space in which we live belongs. In particular, the axioms of plane geometry are true within the limits of experiment on the surface of a sheet of paper, and yet we know that the sheet is really covered with a number

of small ridges and furrows, upon which (the total curvature not being zero) these axioms are not true. Similarly, he says, although the axioms of solid geometry are true within the limits of experiment for finite portions of our space, yet we have no reason to conclude that they are true for very small portions; and if any help can be got thereby for the explanation of physical phenomena, we may have reason to conclude that they are not true for very small portions of space.

I wish here to indicate a manner in which these speculations may be applied to the investigation of physical phenomena. I hold in fact

(1) That small portions of space *are* in fact of a nature analogous to little hills on a surface which is on the average flat; namely, that the ordinary laws of geometry are not valid in them.

(2) That this property of being curved or distorted is continually being passed on from one portion of space to another after the manner of a wave.

(3) That this variation of the curvature of space is what really happens in that phenomenon which we call the *motion of matter*, whether ponderable or etherial.

(4) That in the physical world nothing else takes place but this variation, subject (possibly) to the law of continuity.

I am endeavouring in a general way to explain the laws of double refraction on this hypothesis, but have not yet arrived at any results sufficiently decisive to be communicated.

March 7, 1870.

The President (Professor CAYLEY) in the Chair.

New Fellows elected:

W. G. ADAMS, M.A., *St John's College.*

A. T. CHAPMAN, M.A., *Emmanuel College.*

Communications made to the Society:

(1) *On the Centrosurface of an Ellipsoid.* *By* Prof. Cayley.

The centrosurface of any given surface is the locus of the centres of curvature of the given surface—or say it is the locus of the intersections of consecutive normals, (the normals which intersect the normal at any particular point of the surface being those at the consecutive points along the two curves of curvature respectively which pass through the point on the surface). The terms, *normal*, *centre of curvature*, *curve of curvature*, may be understood in their ordinary sense or in the generalised sense referring to the case where the Absolute (instead of being the imaginary circle at infinity) is any quadric surface whatever: viz. the normal at any point of a surface is here the line joining the point with the pole of the tangent plane in respect of the quadric surface called the Absolute; and of course the centre of curvature and curve of curvature refer to the normal as just defined.

The question of the centrosurface of a quadric surface has been considered in the two points of view, viz. 1°, when the terms "normal" &c. are used in the ordinary sense, and the equation of the quadric surface (assumed to be an ellipsoid) is taken to be $\frac{X^2}{a^2}+\frac{Y^2}{b^2}+\frac{Z^2}{c^2}=1$. 2°, when the Absolute is the surface $X^2+Y^2+Z^2+W^2=0$, and the equation of the quadric surface is taken to be $\alpha X^2+\beta Y^2+\gamma Z^2+\delta W^2=0$:—in the first of them by Salmon, *Quart. Math. Jour.* t. II. pp. 217–222 (1858), and in the second by Clebsch, *Crelle*, t. 62, pp. 64–107 (1863). See also Salmon's *Solid Geometry*, 2nd Ed. 1865, pp. 143, 402, &c. In the present memoir, as shewn by the title, the quadric surface is taken to be an ellipsoid; and the question is considered exclusively from the first point of view: the theory is further developed in various respects, and in particular as

regards the nodal curve upon the centrosurface: the distinction of real and imaginary is of course attended to. The new results suitably modified would be applicable to the theory treated from the second point of view: but I do not on the present occasion attempt so to present them.

(2) *On the correct expressions for the resistance which bodies experience, whilst moving in gases and liquids; with a description of the verifying experiments. By* Richard Potter, A.M.

[*Abstract.*]

The mathematical discussion of the resistance to motion which bodies experience whilst moving in fluid media has remained hitherto in a very imperfect state, although it received great attention from Sir Isaac Newton. In the second Book of the *Principia* he discusses the resistance when bodies move in elastic and non-elastic media.

Canton having in 1762 proved the liquids to be elastic as well as the gases, though they are subject to different laws of compressibility, the consideration which Newton adopted for elastic fluids we know now to apply to all fluids. The Problem VII. Prop. XXXV. of Book II. of the *Principia*, the author held to be the problem still to be solved by the instrument of the modern analysis which was undeveloped in Newton's days. It is enunciated as follows:

"Si medium Rarum ex particulis quàm minimis quiescentibus, æqualibus, et ad æquales ab invicem distantias liberè dispositis constet: invenire resistentiam Globi in hoc medio uniformiter progredientis."

This involves the method which the author followed in his paper on Hydrodynamics published in the *Philosophical Magazine* for March, 1851, before he knew that Newton had considered elastic fluids to be constituted of distinct molecules in

discussing such problems. We are led to consider the fluids as consisting of congeries of heavy very small nuclei surrounded each by imponderable existences, as caloric, electricity, &c., and the volume of each molecule depending chiefly, if not entirely, on the calorific atmosphere. When the whole volume of a fluid body is made up of such molecules we must attribute a cube of space to each molecule, since the cube is the only regular figure which will fill all space symmetrically without vacancies, and the faces of this cube are the areas on which the pressures are to be taken which are transmitted to the nucleus through the means of its elastic atmosphere. These cubes will be of the same volume throughout surfaces of equal pressure and temperature, but will continually vary in volume along directions of variable pressure and temperature. An imaginary vertical cylindrical column of the atmosphere supposed of homogeneous constitution will form an illustration of this; the attributed cubes of the molecules being of equal volume in horizontal strata, but varying imperceptibly in the vertical direction.

If at any point in a fluid δs is the perpendicular distance of the center from the faces of the cube, and therefore $2\delta s$ is the length of the edges, and the distance of the centers of contiguous nuclei; then $4\delta s^2$ is the area of each face of the cube, and $8\delta s^3$ is the volume of the cube. If m is the mass of the nucleus and also of the cube, and ρ the density of the fluid at this point and also the average density of the cube, we have $m=\rho\,.\,8\delta s^3$. Let p be the pressure on a unit of area at the given point, and therefore the pressure on each face of the cube is $p\,.\,4\delta s^2$ in equilibrium. When s is the space measured from any fixed point along the line of variable pressure at right angles to the surface of equal pressure where the pressure is p; then if p becomes p' in surfaces of equal pressure similarly situated at a distance $s \pm 2\delta s$, we have, by Taylor's theorem,

$$\pm\,\delta p = p' - p = \pm\frac{dp}{ds}\cdot\frac{2\delta s}{1} + \frac{d^2p}{ds^2}\cdot\frac{4\delta s^2}{1\,.\,2} \pm \frac{d^3p}{ds^3}\cdot\frac{8\delta s^3}{1\,.\,2\,.\,3} + \text{\&c.}$$

When there is not equilibrium, the internal moving force arising from variation of pressure on the opposite faces of any cube will be $\pm\, \delta p \,.\, 4\delta s^2$. If R is the resultant *external* accelerating force acting on the nucleus, mR is the moving force; and if v is the velocity relative to the neighbouring particles of the fluid, we have $m\frac{dv}{dt} = mv\frac{dv}{ds}$ the effective force acting on the molecule. Then by D'Alembert's principle we have

$$mR \pm \delta p \,.\, 4\delta s^2 - mv\frac{dv}{ds} = 0,$$

and $m = 8\rho\delta s^3$;

therefore $$R \pm \frac{\delta p}{\delta s} \cdot \frac{1}{2\rho} - v\frac{dv}{ds} = 0.$$

When $R = 0$ and the problem is such that we may stop at the first term in the expansion of δp, as in the problem of resistances, by substituting and reducing we have

$$\pm\frac{1}{\rho} \cdot \frac{dp}{ds} - v\frac{dv}{ds} = 0,$$

where we must put $\rho = \frac{p}{\kappa}$ for the gases, and $\rho = \frac{\rho_{,}}{1 - cp}$ for the liquids by Canton's law. This expression differs from that found by the ordinary method only in having the double sign, which has never been hitherto found on the methods of Euler, Laplace, Lagrange, or of all those who following them more or less implicitly have considered fluids as continuous homogeneous bodies; so that they have never been able to find more than the *front* resistance which bodies experience whilst moving through fluids. This consideration leads us to conclude that such methods are essentially defective and erroneous, and that the science of hydrodynamics requires the recognition of the atomic or molecular constitution of fluids.

Let $p_{,}$ be the pressure on a unit of area when the relative velocity v is nothing, then for gases $\rho = \frac{p}{\kappa}$, and integrating we find

$$p = p_{,}\epsilon^{\pm\frac{v^2}{2\kappa}} = p_{,}\left(1 \pm \frac{v^2}{2\kappa} + \frac{v^4}{1.2.4\kappa^2} \pm \frac{v^6}{1.2.3.8\kappa^3} + \&c.\right),$$

and therefore

$$\pm(p - p_{,}) = p_{,}\left(\frac{v^2}{2\kappa} \pm \frac{v^4}{1.2.4\kappa^2} + \frac{v^6}{1.2.3.8\kappa^3} \pm \&c.\right),$$

where the + sign refers to the front and the − sign to the back of the plane unit of area, when there is a relative velocity v either by considering the moving fluid as impinging on the area at rest, or the fluid at rest whilst the plane area moves through it in a direction perpendicular to its surface.

When v is not large, stopping at the first term of the expansion, we have the resisting force

$$\text{for the front of the plane } p - p_{,} = p_{,}\frac{v^2}{2\kappa} = \frac{\rho}{2}v^2,$$

$$\text{for the back of the plane } p_{,} - p = p_{,}\frac{v^2}{2\kappa} = \frac{\rho}{2}v^2,$$

or the front and back resistances are nearly equal for slow velocities, but the whole, being the sum of the two resistances, is the double of that hitherto investigated. They act as a force pushing the body back in front and pulling it back behind, as by a force of suction.

Substituting for $\rho = \frac{\rho_{,}}{1 - cp}$ for liquids by Canton's law and integrating we find

$$\pm(p - p_{,}) = \frac{\rho_{,}}{1 - \frac{c}{2}(p + p_{,})} \cdot \frac{v^2}{2},$$

and neglecting the term having the multiplier c (the compressibility being always exceedingly small in liquids), we have the same expressions for the front and back resistances as for the gases with slow velocities. When the moving body is at or near the surface of the liquid there are circumstances requiring attention which can be investigated as follows: let z be the depth of the liquid which produces the pressure $p_{,}$, so that

$p_{,} = g\rho z$, and h the height a body must fall to acquire the velocity v, or let $v^2 = 2gh$, then substituting these in the expressions for the front and back resistances, we have for the front

$$p = p_{,} + \frac{\rho}{2} v^2 = g\rho\,(z + h),$$

and the pressure on the plane area in front increases with the depth z, and with h or v^2 directly. For the back we have

$$p = p_{,} - \frac{\rho}{2} v^2 = g\rho\,(z - h),$$

and the pressure on the back of the plane is nothing for $z = h$, and also for all less values of z. This can be seen in the easy experiment of moving a flat rod, or even a walking stick briskly through the water, when to a certain depth it will be seen that air follows the rear of the rod, and to a certain depth there is no pressure of water on the rear of the rod. This experiment, in accordance with theory, shows that experiments for the resistances which bodies such as ships and boats moving at the surface of water experience must be tried at the surface, and not through the body of the fluid.

Having investigated the above expressions for the front and back resistances of a plane moving in a direction perpendicular to its surface, the rest of the investigations for the resistances of bodies of different forms are similar to those used in elementary analytical treatises on hydrostatics and hydrodynamics. Thus, if a small plane area moves obliquely in a fluid, let θ be the angle between the direction of motion and the perpendicular to the plane, then the velocity perpendicular to the plane is $v \cos\theta$, and the perpendicular resistance varies as $v^2 \cos^2\theta$, and this resolved in the direction of the motion is $v^2 \cos^3\theta$. When a solid of revolution moves in the direction of its axis of revolution, taken for the axis of x, in a fluid, we have the area of an elementary ring equal to $2\pi y ds$ if s is the arc of the curve by whose revolution the front part of

the surface is generated with the ordinate y, and $\cos\theta$ becomes $\frac{dy}{ds}$.

If we take s' for the arc of the hind part of the body, we have the area of the elementary ring of the same radius y equal to $2\pi y ds'$, and $\cos\theta'$ for the hind part equals $\frac{dy}{ds'}$. If we put μ and μ' for the coefficients of v^2 at the front and back respectively, we have the resistances experienced by the elementary rings of radius y equal to

$$2\pi\mu v^2 y \frac{dy^2}{ds^2} ds = 2\pi\mu v^2 y \frac{dy^2}{ds^2} dy$$

for the front, and

$$2\pi\mu' v^2 y \frac{dy^2}{ds'^2} dy$$

for the back of the body.

Integrating the sum of these, we have the resistance experienced by the solid of revolution moving in the direction of the axis equal to

$$2\pi v^2 \left\{\mu \int y \frac{dy^2}{ds^2} dy + \mu' \int y \frac{dy^2}{ds'^2} dy\right\}.$$

This expression becomes simplified when the front and back surfaces are the same, or when one of them is plane, and gives the following results.

The previous investigations show that both in gases and liquids for moderate velocities the values of μ and μ' are nearly equal and each equal to $\frac{\rho}{2}$ nearly. Taking $\mu = \mu' = \frac{\rho}{2}$, we find the following:

Ex. 1. To find the resistance experienced by a circular disc or short cylinder, moving in a fluid with a velocity v in a direction perpendicular to its plane.

When b is the radius of the disc, then the whole resistance front and back $= \pi\rho v^2 b^2$.

Ex. 2. To find the whole resistance experienced by a sphere of a radius b moving in a fluid with a relative velocity v.

We find the resistance $= \frac{1}{2}\pi\rho v^2 b^2$, which is half that found for a plane circular disc in the previous example considered as a great circle of the sphere. This resistance experienced by a sphere is, however, the double of that found in the treatises on hydrodynamics hitherto published.

Ex. 3. To find the resistance experienced by a hemisphere of a radius b moving in a fluid in a direction perpendicular to its plane surface, but with either the plane or curved surface first.

We find the resistance $= \frac{3}{4}\pi\rho v^2 b^2$ in each case, which is half as much again as that experienced by the whole sphere.

Ex. 4. To find the resistance experienced by a spheroid moving in the direction of its axis of revolution in a fluid.

Let a be the axis about which the revolution takes place in the formation of the surface, and b the other axis of the generating ellipse.

Then the resistance

$$= \pi\rho v^2 \frac{b^4}{a^2 - b^2}\left\{\frac{a^2}{a^2 - b^2}\log_e\left(\frac{a^2}{b^2}\right) - 1\right\},$$

from which the results of Examples 1 and 2 as particular cases may be obtained by expansions for prolate and oblate spheroids.

Ex. 5. To find the resistance experienced by a segment of a right cylinder on an elliptic base moving in a fluid in the direction of the major axis of the ellipse.

If a be the axis of the ellipse in the direction of the motion,

b the other axis of the ellipse, and $e^2 = 1 - \frac{b^2}{a^2}$, also f equal the height of the cylinder, then the resistance on the whole surface

$$= 2\rho v^2 fb \cdot \frac{1-e^2}{e^2} \left\{ \frac{1}{e\sqrt{1-e^2}} \sin^{-1} e - 1 \right\}.$$

Ex. 6. To find the resistance experienced by a right cone moving in the direction of its axis with either the apex or the base first.

Let a be the height and b the radius of the base of the cone, then the resistance on both surfaces

$$= \frac{1}{2} \pi \rho v^2 b^2 \left\{ \frac{b^2}{a^2 + b^2} + 1 \right\},$$

which diminishes as a increases, and becomes $\frac{3}{4} \pi \rho v^2 b^2$ when $a = b$, which is the same as for the hemisphere.

Ex. 7. To find the resistance experienced by a double cone moving in the direction of its axis in a fluid.

Let a be the height of each of the cones, b the radius of their common base.

Then the resistance $= \pi \rho v^2 \cdot \frac{b^4}{a^2 + b^2}$. This is the same as for the sphere when $a = b$ and b is the radius of the sphere.

Experiments were described which verify the results of the front and back resistances being nearly equal, and show the effect of different forms of bodies moving round an axis in water.

March 21, 1870.

The President (Professor CAYLEY) in the Chair.

New Fellows elected:

R. PENDLEBURY, B.A., *St John's*.
E. F. EDWARDES, B.A., *Trinity*.

The Treasurer (Dr CAMPION) presented his accounts, and

congratulated the Society on the improved state of their finances.

The thanks of the Society, proposed by Professor BABINGTON, and seconded by Professor SELWYN, were cordially accorded to him.

Communications made to the Society:

(1) *On Carmine and the colouring principles of Cochineal. By* Mr RÖHRS, *Jesus College.*

In most treatises on Chemistry it is stated that carmine is a compound of a peculiar acid styled carminic acid, and animal matter, to which for the purpose of giving body to the pigment alumina or oxide of tin are occasionally added.

Numerous receipts for its manufacture have been published; in an early edition of "Ure's Dictionary of the Arts" there are several; and it was from one of these that I hit upon the clue to the correct theory and practice of carmine-making. In all these receipts rain or distilled water, or *river water* are recommended to be used. With rain or distilled water I invariably found failure to attend my experiments, and with river water also, unless under peculiar circumstances. These circumstances were, the presence of a notable quantity of lime in the water, existing I believe in the form of carbonate of lime dissolved in carbonic acid. This led me after numerous experiments to devise the following method of making carmine (on the small way and for the use of artists and amateurs, though I see no reason why it might not be carried out on a larger scale) which yields a pigment of exquisitely pure scarlet tint and of a richness and brightness beyond any to be now met with in the shops.

The first condition to be observed, is that the cochineal should be of the right sort. It should be by preference a Mexican black, or silver grey, hard and shelly, of rather a small starved shrivelled grain; not pasty when ground. The colour of the ground cochineal ought to incline to foxy red, and not to purple.

Honduras black also yields a good carmine, but I prefer the Mexican cochineals.

Take 840 grains of the best cochineal; grind it well in a porcelain, not metal, mortar to a powder of the fineness of ground coffee or snuff. Intimately mix with it by grinding 40 grains of whitening, *i.e.* washed chalk or carbonate of lime. Set a 5 quart tinned iron saucepan, size No. 8, filled to about an inch of the top with rain water, on the fire to boil. This will contain about 4 to 4½ quarts. Before boiling, throw in 8 grains of bicarbonate of ammonia (carbonate of ammonia of the shops exposed for a few days to the air), and 5 grains of oxalate of ammonia (to precipitate any lime that the rain-water may have taken up from the roof of the house); when the water boils remove the saucepan from the fire, add about half an ounce of cold rain-water to throw the "water off the boil," then stir in the mixture of cochineal and whitening. Replace the lid, and leave the saucepan for about 8 or 9 minutes near the fire on the fender, so as to keep the temperature as nearly constant as possible, and just a little under the boiling point. At the end of the 8 or 9 minutes sprinkle in slowly, little by little at a time, stirring all the while with a clean wooden rod, from 37 to 40 grains of finely powdered chemically pure alum. Then transfer the contents of the saucepan to a conical tin vessel, provided with a handle and lip; a milk-can will answer very well. This vessel ought previously to have been warmed with scalding water. Cover the can with a wooden or pasteboard cover, or the lid of a cigar box, &c., muffle the whole up in a cloth to keep in the heat, and leave it just outside the fender in front of the fire for about from 15 to 20 minutes. By this time, to quote from Ure, the "bath will be as clear as though it had been filtered," and of a bright scarlet colour. Decant cautiously about ⅔rds of the contents into a clean tinned iron saucepan, add about half an ounce fluid measure of a solution of the white of one egg in 16 ounces of water (the solution must previously have been strained through

a muslin sieve), so that about $\frac{1}{30}$th of the white of egg will have been added (or even less may be tried at first), this must be well stirred in. Set the saucepan on the fire to warm, not boil, interposing the tongs between the heated coals and the bottom of the saucepan. As soon as the saucepan begins to sing, it must be removed and examined, if the liquor be not yet curdled the saucepan must be replaced on the fire, but if curdled, the contents must be stirred and transferred to a clean tinned vessel of the milk-can form, set in a basin of cold water, to cool, and left for about twenty minutes. At the end of this time the carmine will have gone to the bottom, and the supernatant liquor may be decanted off, and kept for purposes presently to be described. The carmine will now be collected on a filter, washed with a little pure rain-water, scraped off with a silver spoon, and set on a cushion of filtering paper to dry in a dark closet. The greater part of the remaining one-third of the scarlet liquor may be decanted cautiously off the dregs and by means of two or three decantings freed from uncombined chalk and animal matter. It will yield a carmine of good quality but not so good as the first two-thirds. The carmine obtained from the first two-thirds will be of a magnificent geranium scarlet, lighter and brighter with 37 of alum than with 40, but perhaps richer with 40. I do not know where carmine so brightly scarlet can now be met with, though the best carmine made many years ago by the then well-known chemical and colour manufacturers the Bergers, and which, notwithstanding its high price, enhanced no doubt by a protective duty, was found so superior to any French carmine, as to be largely used by the artificial flower makers at Paris, was to say the least as bright as what I have made; but there is no longer such good cochineal to be met with, as was the case then. However, I do not think that carmine is to be purchased now so bright as that made by my own method, which though based on the French method described in Dr Ure's dictionary is essentially different in its

details, especially in the precautions taken to avoid overheating the cochineal, and above all in the addition of whitening, without which it would fail entirely.

I will now detail a few experiments which I think will throw a good deal of light on the theory of carmine making; and establish the fact of the existence of at least *two* distinct colouring principles in cochineal.

(1) If no whitening be added to the cochineal, nothing but a small quantity of a dirty purplish, precipitate is obtained with *rain*-water. If an insufficient quantity such as 8 or 10 grains only instead of 40, carmine will be produced, but of a bad quality, and of a dull crimsonish colour without lustre. If 25 to 30, carmine as good or nearly so as with 40, the excess above what is necessary to decompose the alum and combine with the colouring principle going to the bottom with the dregs after the alum has been added.

(2) If carbonate of barytas or carbonate of magnesia be substituted for the whitening, dirtycoloured purplish precipitates are the result.

(3) If a small quantity of ordinary carmine be burnt in a silver spoon over the flame of a spirit lamp, and the ashes digested in dilute nitric acid, filtered, and to the filtrate oxalate of ammonia and ammonia sufficient to neutralize the liquor be added, a copious precipitate falls, consisting of oxalate of lime and alumina, of which the alumina is taken up again on the addition of acetic acid.

(4) If carmine be dissolved in liquor ammoniæ diluted with about 3 times its weight of water, and the liquor filtered, a residue consisting apparently of lime and alumina remains.

(5) If the filtrate be rendered strongly acid by acetic acid, a copious precipitate ensues of what is known by the name of "precipitated carmine."

(6) If to the filtrate from which the precipitated carmine was separated, oxalate of ammonia be added, a precipitate of a

considerable quantity of oxalate of lime results, and if this filtrate be rendered neutral by ammonia and acetate of lead be added, a precipitate of a beautiful *bluish purple* ensues.

(7) If the precipitated carmine be collected, washed with a little cold water, (it is soluble in excess of boiling water) and again dissolved in liquor ammoniæ, and to the solution acetate of lead be added in sufficient quantity, a precipitate of a *reddish* purple or purplish red is obtained.

(8) This precipitate may now be carefully washed and mixed with water; the quantity obtained from about 100 grains of ordinary carmine may be mixed with about 4 ounces of rain-water, and decomposed by sulphuretted hydrogen gas. This operation requires great care, as the peculiar colouring principle or acid, and which I shall call coccineo-carminic acid, and which is here combined with the oxide of lead, is easily decomposed, and partly converted into another acid, the *purpureo-carminic* acid, the same as that combined with the lead in (6).

It is best to operate with a small apparatus composed of two six ounce bottles, containing one the sulphide of iron, and the other the creamy mixture of the coccineo carminate or, for short, c. carminate of lead. A tube ¼ inch thick externally is wide enough for the tube through which the gas passes, the bubbles of gas should not come through faster than about 3 in a second, and the operation should be arrested at the end of about 10 minutes or a quarter of an hour at most; any way, the operation must be arrested before the whole of the lead is saturated, and some of the c. carminate of lead must be reserved apart to mix with that in the bottle at the end of the operation to absorb any sulphuretted hydrogen that may be dissolved in the liquor.

I fancied that the operation succeeded better when the air was excluded as much as possible from the bottle containing the c. carminate of lead, by means of a little blotting paper loosely stuffed in at the neck.

When the operation is finished, and after the contents of the

bottle have been well shaken up with a little fresh c. carminate of lead, they must be boiled in a porcelain evaporating dish and filtered boiling hot through a double filter of fine filtering paper, and the filter washed once or twice more with boiling water, and the matter on the filter collected and boiled over again and filtered again and washed with boiling water, as the c. carminic acid is but sparingly soluble even in hot water, and adheres obstinately to the sulphuret of lead.

(9) The filtrates may then be mixed together, and the liquor rendered strongly acid by acetic acid, this will cause after some time, or immediately if the liquor be warmed, a precipitate of pure c. carminic acid, combined perhaps with a trace of gelatinous matter, this collected and dried in the shade resembles closely precipitated carmine, which indeed consists almost entirely of c. carminic acid, with the addition of a little alumina.

(10) If now to the liquor from which carmine has been extracted in the process of carmine making, a few drops of a solution of acetate of barytas be added to precipitate any sulphuric acid that may be present derived from the alum, and about an ounce of common acetic acid to precipitate any c. carminic acid that may still remain uncombined with the carmine, and the liquor be filtered, and the filtrate rendered neutral by ammonia, and acetate of lead in sufficient quantity be added, (about 150 grains for the 840 of cochineal is ample), a beautiful bluish purple or purplish blue precipitate of purpureo carminate or (for short) p. carminate of lead ensues, this must be collected on a filter and well washed.

(11) If now this precipitate (which seems to be as rich and beautiful a pigment in its way as carmine itself) be treated as the c. carminate of lead was, with sulphuretted hydrogen only, the gas may with advantage be passed through it for an hour or more, some fresh p. carminate being kept apart to absorb any sulphuretted hydrogen in the bottle at the end of the operation, and the liquor be filtered, washed with a little water, and the

concentrated filtrate dried in a water bath, a quantity of a crystalline mass of p. carminic acid is obtained. The p. carminic collects like a dense brownish purple glaze on the evaporating dish of the water-bath, and on being scraped off with a sharp knife, assumes the form of a crystalline powder, full of shining scaly crystals. It is very soluble, and the colour of the solution a purplish red, or yellowish red, if all the sulphuretted hydrogen has not been absorbed and any part of it left to be transformed into sulphurous and then sulphuric acid.

If to the solution of p. carminic acid a little ammonia be added, the liquor assumes a rich lake or wine colour very different from the deep crimson of the solution of c. carminic acid in ammonia.

(12) If to the solution of common carmine in ammonia diffused through sufficient water, freshly precipitated gelatinous alumina be added, a beautiful crimson lake of the colour of a crimson rose or boiled red beet-root is obtained; but if to the solution of p. carminic acid in ammonia, or to the carmine liquor neutralised after all the carmine has been got out of it, gelatinous alumina be added, a lake-coloured lake, paler, purpler, and weaker and altogether of another tint, is the result.

(13) If the liquor in (9) from which the c. carminic acid was precipitated by acetic acid be neutralised, and then acetate of lead be added, a copious precipitate of the p. carminate of lead follows.

(14) The p. carminate of lead is decomposed by phosphoric acid, and p. carminic acid results, but from the few experiments that I made, I inferred that not the *whole* of the p. carminate was thus acted on, only a part, and a reddish precipitate of a mixture probably of phosphate of lead and a double phosphate and p. carminate of lead remained.

(15) Sulphuric acid seemed entirely to decompose the c. carminate of lead, and the c. carminic acid it contained as well, a variable quantity of p. carminic being formed at the same

time; but as the results of the few experiments which I made with phosphoric and sulphuric acid on the p. carminate and c. carminate were so unsatisfactory, I did not continue them, and did not very carefully examine the products to which they gave rise.

From these experiments I infer, first that two distinct colouring matters exist in cochineal, the c. carminic acid and the p. carminic acid. That the first is extremely unstable and easily decomposed by heat and strong acids and even ammonia, the result of the decomposition being in part the second acid. For I succeeded in obtaining p. carminate of lead of the bluest tint most free from any c. carminate by boiling, for an hour or two, cochineal in water, adding acetic acid to the decoction and filtering, neutralising with ammonia, and adding acetate of lead; the boiling, as much as the addition of the acetic acid, contributing to the elimination of the c. carminic acid, by decomposition. The p. carminic acid is soluble in acids, but the c. carminic acid is nearly if not quite insoluble in dilute acetic acid even at a boiling temperature. The addition of chalk to the cochineal may act partially by decomposing the alum, and the sulphate of lime so formed may prevent the fatty acids in the cochineal from being dissolved, and soiling the carmine; but as for this purpose carbonate of magnesia would answer as well, and as it did *not*, and as lime was certainly contained in the carmine I examined, carmine I bought, as well as in my own, I infer that it is an essential ingredient of that colour.

Hence we see too why owing to the extreme instability of c. carminic acid, and to the fact that heat decomposes it, it is so necessary to avoid long-continued boiling in the preparation of a bright carmine, and why it is prudent to use so little alkali, and that saturated with carbonic acid in the operation. The excess of carbonate of lime by rendering the liquor neutral no doubt tends also to prevent the c. carminic acid from being precipitated to the bottom among the dregs, and allows of the slow

formation of the c. carminate of lime and alumina which, I take it, is the most essential part of carmine, if not all that carmine consists of; but if that were the only use of the lime, carbonate of magnesia or of barytas would answer as well, and as I have just observed, they do not. As to the salts of tin, the chlorides throw down crimson and reddish *lakes*, drying up into hard brittle substances from both the p. and c. carminates of ammonia. The c. carminic acid is probably decomposed by bichloride of tin. Actinic influences have nothing to do with the success of the operation; I have made as good carmine in dull days as in bright ones, nor, if the vessels are clean, as they should be, (for that is of primary importance, the slightest rust or impurity being evidenced by a degradation in the tint of the product,) is failure possible. The notion that fine weather is favourable to carmine making, may arise from the fact, that in fine dry weather, river water holds more lime in solution than when diluted by copious rains. With the exception of the carmine made years ago by the English firm of Berger, the best I believe has been made in France, and at Rouen, and the river Seine is saturated with chalk. The latest investigation into the colouring matter of cochineal is I believe that published by De la Rue. His carminic acid must (I imagine from the résumé of his results given in "Chemistry as applied to the Arts" by Dr Muspratt) be the same as what I call purpureo carminic acid.

The combining numbers of c. carminic and p. carminic acids would be best determined from their lead compounds, but for this purpose an accurate Liebig's apparatus and a good pair of scales are necessary, and my appliances are of the rudest and homeliest kind. I cannot lay claim to any great skill or experience as a practical or even theoretical chemist, but my experiments imperfect as they are, will I think be sufficient to prove the thesis I have advanced, viz. that of the existence of two colouring principles in cochineal, and of lime in carmine, and may perhaps draw the attention of abler chemists than

myself to this interesting subject. I forgot to state that with good cochineal the $\frac{2}{3}$rds of the liquor from which the best carmine is made yields about 45 grains of carmine to 840 of cochineal; 40 grains is however the general average. Carmine is crimson viewed by *transmitted* light, and scarlet by reflected light; for water-colour painting as little gum as possible should be employed, barely enough to prevent the carmine when dry from rubbing off on the finger, otherwise its vivid colour will be much degraded; carmine and drawings coloured with carmine should be kept in a dark place.

Professor LIVEING mentioned some investigations, not noticed by the author, which were in favour of the theory of two colouring principles.

(2) *On a Roman Lanx and other Antiques found at Welney. By* MR LEWIS, *Corpus Christi College.*

Having lately spent a few days in the fen country lying between Ely and Huntingdon, I was much surprised with the variety of interesting questions suggested on the most cursory survey of what is described in county histories as a dreary and uninteresting plain. Year by year the ploughman brings again to light huge trunks of sound but blackened oak; acorns and hazel-nuts, as of last year's growth; horns of red deer, perfect as when shed by the monarchs of the woodlands, who shall say how many centuries ago—objects these which tell of a primeval forest age which must have been succeeded by alternations of submergence and states of rank swampy vegetation, for in many parts horizontal seams of alluvial soil are found dividing deep layers of peat. The progress of agriculture having by arterial drainage made fertile for corn and grass spots formerly little visited, except for the flocks of wild fowl to be shot or the curious butterflies to be caught, we are able at last to begin our conjectures as to the mode of life

of the (may I say?) antediluvian inhabitants of this fen country on which in later times the old Roman castle of Camboritum looked down as a peninsula on a number of islets. In order at once to consolidate and manure the upper stratum of peat which years of drainage have reduced to less than half its original thickness (within 40 years a subsidence from nine to four feet in depth has been observed), the farmer every eight or ten years spreads on the surface and ploughs in a layer of stiff clay brought up by means of trenches three feet wide at intervals of about 15 yards. Rarely are these clay-pits opened without disclosing not only the vegetable and animal traces of ages past, which I have mentioned, but also implements of flint, bronze, and iron, which admit of close comparison with those already classified by the laborious skill of M. Keller, Sir John Lubbock, and other pre-historic archæologists. Nor are clear evidences of Roman occupation wanting, not only as elsewhere in bulwarks against a common enemy, the ocean (*e.g.* at Lynn), and in roads (*e.g.* that from Denver through March to Peterborough), but again and again, as an elevation of a few feet above the surrounding fen finds us on what is still an island, speaking for itself in such names as Ston-ey, Angles-ey, &c., we discover that never-failing evidence of a Roman habitation, pottery, as well as arms and domestic appliances for use and luxury. It is an object of the last class which by the favour of the owner, Mr A. Goodman, I have now the honour of submitting to your criticism—a charger which, for reasons that may prove satisfactory, I think may be considered of Roman work. It was found in the spring of 1864, at the depth of 14 inches, in the course of gault-ploughing a piece of old grass land, about 200 yards from the Hundred Feet River at Welney, once an islet in the district of Wella, which now comprises the parishes of Upwell, Outwell, and Feltwell, in the county of Norfolk. Various opinions have been suggested in regard

to the original intentions or use of the large disc of metal thus brought to light; there can, however, be little doubt that it is a specimen of the flat charger or dish used by the Romans to hold a large joint of meat, or, as in a case mentioned by Horace (*S.* II. 4. 41), a boar entire (illustrated and confirmed by an ancient fresco found near S. John Lateran at Rome), and also serving occasionally for sacrificial rites (Virg. *G.* II. 194, &c.). Such an appliance of the table was properly designated a lanx, and the epithets, "panda," "cava," and "rotunda," commonly applied to it by ancient writers are obviously most appropriate. To the kindness of Professor Liveing I owe an analysis which shews that the metal of this lanx is 80 per cent. tin, with 18½ lead, and a little trace of iron, thus nearly corresponding with the argentarium of Pliny (*H. N.* XXXIV. 20 and 48), and with certain oval cakes that have been found in the bed of the Thames, near Battersea, on which are stamped the Christian monogram, with the word "spes," and the name, as it is believed, of Syagrius, perhaps the same whom we hear of as secretary to the Emperor Valentinian. One of these cakes weighs nearly 111 ounces. In the term ἑανὸς κασσίτερος, as designating the material of which Hephæstus made the greaves of Achilles (*Il.* XVIII. 612), we probably find the earliest mention of a compound of this kind; and Boeckh (*Inscrr.* I. 150, § 48) gives καττιτέρινα ἐνῴδια (ἐλλόβια?) πέντε as occurring in a list of offerings of plate and jewelry dedicated, in *Ol.* 95. 3 (B.C. 398), to the gods of Athens. The pliability of such metal shows the strong propriety of the words of Juvenal (V. 80)—

> "Aspice quam magno *distendat* pectore lancem
> Quæ fertur domino squilla."

As is seen also in the passage from Horace quoted above. Of the lanx before us the diameter is 2 ft. 4¾ in., equal to 2½ feet of Roman measure, the weight 30 lbs.—excessive, according to our modern ideas, of the capabilities of servants; but Pliny

(*H. N.* XXXIII. 52) tells of a Spanish dispensator (one of the slaves of the Emperor Claudius) who had a lanx of 500 lbs. weight and eight more of upwards of 100 lbs. each, to make a complete service (if such be the true meaning of "Comites ejus octo octengentarum et quinquagenta librarum)," and naively adds, "Quæso quam multi eas conservi ejus inferrent aut quibus cœnantibus." Tertullian, alluding to the passage, calls such a dish "promulsis," meaning, I suppose, "promulsidarium." In the centre, on the upper surface, which is slightly dished to prevent the gravy flowing over, there is a circular compartment nearly nine inches in diameter, encircled with a very elaborate diapered pattern of peculiar type, produced apparently by means of the punch, chisel, and hammer: this compartment, of which the beautiful design is somewhat indistinct in the present condition of the surface, is surrounded by a bordure, decorated with trailing or branched work, in the outer circle of which may be discerned, in ten spaces, at equal intervals, certain letters, which my learned friend, Mr A. Way (who has materially aided me in the present investigation), thinks form the words VTERE FELIX; but I fear that I cannot state more in support of this view than that there is certainly a V and an X, with eight intersecting arcs between them, and that in several of the intersections the letter E may be distinguished. Some of the letters appear to be deeply incised, while others are embossed in slight relief. On the reverse of the lanx there is a central circle in relief, possibly thus fashioned to give more substantial support and prevent the risk of bending or falling out of shape that might occur in so large a flat plate of metal when a heavy joint of meat was carried upon it. Although but few examples of the lanx are to be found in collections of Roman utensils, they have occasionally occurred. A large plane lanx of silver was acquired for the British Museum in the Blacas collection, and in Lysons' *Reliquiæ Britannico-Romanæ*, Vol. I., we find one figured of

similar style and composition, but inferior in size and quality of decoration, which was found near Manchester on the site of Mancunium, with two others—the three measured in diameter $14\frac{7}{8}$ in., $17\frac{3}{4}$ in. and 20 inches, or $1\frac{1}{3}$, $1\frac{1}{2}$, and nearly 2 Roman feet. I have failed to ascertain the existence of any pewter vessels of Roman work in Continental Museums. Their manufacture may have been exclusively carried on in Britain. It may well be worth while to consider if chemistry will not supply us with some agent that shall arrest the exfoliation, which has already done so much to mutilate the surface. Besides numerous oak trunks, the only other objects discovered on the same estate are the small weapon at hand, probably one of the earliest forms of dagger known in the bronze age, and the well-preserved antler before you: but the present tenant, Mr G. Daintree, having promised ere long to make a careful sounding of the whole field in which the treasure before us once more saw the light, I have little doubt but there is a rich and instructive harvest yet in store for the classical antiquary, especially as vases full of Roman coins have been found in the adjoining parish of Upwell.

Prof. C. C. Babington remarked on a shield which was also on the table, calling attention to the absence of the usual tacks, which served for the attachment of leather for additional strength. He also gave instances of pewter vessels being found in the Thames. He commented on some of the other antiques on the table, amongst which were two stone weapons (polished) and a Roman statuette (bronze). He also called the attention of the finders of such things to the great importance of depositing them at once in some Museum, such as that of the Cambridge Antiquarian Society, where they would be safe, instead of being ultimately made toys for children and destroyed, as now was often the case.

Mr Lewis made some further remarks on the localities where the various antiques had been found, calling attention

also to a bow which was said to have been found in the fens.

Mr BONNEY expressed his opinion that the inscription on the lanx was not UTERE FELIX, though he was unable to suggest any other reading. He called attention to an antler of a red deer on the table exhibiting cuts, and doubted whether the bow could have been found in the fen.

May 2, 1870.

Mr TODHUNTER (*Vice-President*) in the Chair.

New fellow elected:

CLEMENT HIGGINS, B.A. *Downing*.

Communications made to the Society:

(1) *On the best form for the ends of Measures à bouts. By* Professor MILLER, F.R.S.

After describing the various forms which had been commonly adopted, and pointing out their defects, the author stated that the best form was that of two 'knife edges,' whose edges were in planes perpendicular to each other, and were not straight lines, but arcs of circles, whose centre was the opposite end of the axis of the bar. In this case if l were the length of the bar, e the distance between the real and the assumed position of the point where the axis of the bar intersects the bounding surface, the amount of the error was $\frac{e^4}{4l^3}$; while in the forms commonly used it was $\frac{e^2}{l}$ or $\frac{2e^2}{l}$; either of which quantities were considerably greater than the error in the form proposed by the author.

(2) *Note on supposed Mollusc borings in the carboniferous limestone of Derbyshire. By* T. G. BONNEY, B.D.

The burrows described were in two localities, the first was in a band of limestone which crops out between two sheets of

toadstone on either side of Salter's Lane above Matlock Bridge; at a height of more than 400 feet above the river Derwent. Here the burrows occur in great abundance. A specimen of these was exhibited; and the author contended that, from the shape and arrangement of the burrows, it was impossible that they could be the work of *Pholades*. The second case described was at the bottom of Miller's Dale, in the upper part of a low cliff by the road side, about 12 feet above the present level of the river Wye. The author believed that this cliff was an artificial scarp formed in making the road. But even if the cliff were natural, he pointed out that Miller's Dale was distinctly a valley of fluviatile erosion, that there was no indication of its having been submerged in its present form beneath the sea; and consequently that these burrows, so near the bottom of a gorge of this kind, could not be the work of any marine mollusc. He was therefore convinced that the *Pholas* theory was untenable; and believed that the burrows were excavated by *Helices*.

May 16, 1870.

The PRESIDENT (PROFESSOR CAYLEY) in the Chair.

Communications made to the Society:

(1) *Helmholtz and Tyndall on the Theory of Musical Consonance. By* Mr SEDLEY TAYLOR, M.A.

[*Abstract.*]

The *quality* ('timbre') of musical sounds in general has been shewn by Helmholtz to depend on the relative intensity in which the partial tones, of which they consist, are present. Dissonance depends on the occurrence of *beats* between adjacent sounds. When two fundamental tones are so related to each other that beats are not produced between any well-developed pairs of the corresponding over-tones, the interval between the fundamental tones is *consonant*.

In the case of *simple* tones, *i.e.* of such as possess no overtones whatever, the difference between consonance and dissonance depends on the presence of a class of sounds discovered by Sorge in 1740, and generally since known as Tartini's tones, but called by Helmholtz, who has considerably increased the series, *combination-tones.* In the case of two simple tones the interval is consonant if no audible beats are produced by combination-tones with the primaries or with each other—dissonant, if otherwise.

Professor Tyndall, in the last of his published Lectures on Sound, has given a theory of consonance which differs radically from that of Helmholtz, and is irreconcilably at variance with experiment. The fundamental error of his reasoning consists in the neglect of the most essential condition for the production of audible beats between two simple tones, namely, that they must *lie near each other in the musical scale.*

(See for the proof of this in detail a Letter by the writer of this paper in 'Nature' for March 3, 1870.)

Professor CHALLIS made some remarks vindicating Dr Smith and Young from a statement made by Prof. Tyndall, with regard to the theory of consonance not being understood before the time of Helmholtz, and expressed a general concurrence in Mr Taylor's paper, so far as he had followed up the subject.

Mr TROTTER said that Helmholtz had first assigned to combination-tones their true origin. When two loud tones coexist the excursions of the molecules are so large that terms of the second order arising from the combination of the vibrations become sensible.

Mr PALEY remarked upon the beats heard when a church bell has been struck, and the tone is dying away; thinking that this might result from the tin and copper not being well amalgamated in the metal.

Mr TAYLOR, followed by Mr TROTTER, said that these beats were produced by the bell not being in perfect tune throughout,

and Professor STOKES gave the same explanation, pointing out that a bell was rarely a mathematical figure of revolution.

Mr POTTER attributed these beats to the occurrence of nodal lines on bodies when sounding, which gave rise to interferences.

(2) *On a case of Asymmetry in the Human Body. By* Professor HUMPHRY, F.R.S.

The subject of this paper was a female in Addenbrooke's Hospital, who was born with one side of the body on a larger scale than the other, the want of symmetry being complete throughout. For example, it amounted to 2½ inches in length of arm, and had been carried on in the right mammary gland and right side of the face, and even in the tonsil and teeth, the teeth being in a plane a little lower than on the left—the right arm and right leg stronger than left in either case; the person was in good health and well made, with not the slightest sign of paralysis. Prof. Humphry quoted a case mentioned by Broca, of a boy, aged 11, in whom asymmetry was very marked, so much so as to look as if he were made of halves of different persons put together. He also exhibited two models of a brain from Van der Kolk's museum at Utrecht, where there was marked want of symmetry. In this case there had been paralysis of the *opposite* side of the body; here it would result from deficiency of growth. It would be interesting to know whether the right or left side of the brain in the present case were larger.

A conversation took place concerning the symmetry of crystals, in which Professors MILLER, HUMPHRY, and CAYLEY joined. Mr SEELEY asked whether temporary paralysis of the mother would account for the asymmetry exhibited by the subject of the paper; Dr HUMPHRY doubted whether it would do so.

May 30, 1870.

The PRESIDENT (PROFESSOR CAYLEY) in the Chair.

New Fellow elected:

F. S. BARFF, M.A. *Christ's College.*

Communications made to the Society:

(1) *On the Invention of the Camera Lucida by Wollaston. By* Professor MILLER, F.R.S.

Among a collection of scientific instruments placed under the care of the University by Mr Elphinstone, Professor MILLER had found two glass prisms cemented together and a four-sided prism of glass, together with other combinations of lenses and prisms; to some of which reference was made by Wollaston in two or three papers in the Philosophical Transactions. Professor Miller showed how these instruments might be used in discovering and approaching to the regular form of the camera lucida.

(2) *On the Frontal Bone in the Ornithosauria; with additional evidence of the structure of the hand in Pterodactyles from the Cambridge Upper Green Sand. By* Mr H. SEELEY, F.G.S.

The portions of the head hitherto obtained from the Cambridge Upper Green Sand are the back part and front part of the mouth. The specimen exhibited was part of the frontal bone, which showed that the Pterodactyle approached closely to the avian type. Mr SEELEY had for some time supposed that the fingers of the Pterodactyle had been misplaced, and the descriptions usually given and the specimens exhibited, in his opinion, fully confirmed that suspicion.

(3) *Note on a new species of Plesiosaurus from the Portland Limestone. By* Mr H. Seeley, F.G.S.

Some time back a specimen of the fore limb of a Plesiosaur was obtained from the Portland limestone for the British Museum. Remains had also been found which had been referred to Pliosaurus. The specimen of two vertebræ which was exhibited was undoubtedly a Plesiosaur, but appeared to Mr Seeley to show certain approaches to the Pliosaurian type.

(PART XII.)

PROCEEDINGS

OF THE

Cambridge Philosophical Society.

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Cambridge:
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October 31, 1870.

PROFESSOR CAYLEY (PRESIDENT) in the Chair.

The following Officers were elected:

President.

Professor CAYLEY.

Vice-Presidents.

Professor SELWYN.
Mr TODHUNTER.
Professor BABINGTON.

Treasurer.

Dr CAMPION.

Secretaries.

Mr BONNEY.
Mr J. W. CLARK.
Mr COUTTS-TROTTER.

New Members of the Council.

Professor LIVEING.
Mr PALEY.
Mr DANBY.

The Vitality of Paganism, an Exposition of the Doctrines of the Nuseiríyeh, a Secret Sect in Modern Syria. By E. H. PALMER, M.A., *St John's College.*

[*Abstract.*]

The paper sets out with the theory that all unrevealed religion originated in the worship of light or space, i. e. the Sky, as the

only symbol by which the conception of an omnipresent and all-pervading deity could be arrived at. The worship of the Sun and Moon are only successive developments of the same idea and symbols of what was at first itself a symbol. The five planets are next deified, and in these eight we have the germ of all mythological systems. The Sabæan religion was actually Paganism in this stage. The worship of the Sun as a corruption of the earlier worship of the Sky is hinted at in the Greek myth of Zeus supplanting *Kronos the son of Ouranos* as King of Heaven. The origin of the ancient mysteries was the preservation of the esoteric doctrine of Paganism; an attempt to recall to people's minds the fact that their deities but represent the heavenly bodies, which again are only the symbols of light; and that the latter is but an expression for the conception of an all-pervading God. Such secret teaching has always existed in the East; and as the mystic sects of Syria and Persia at the present day inherit the traditional explanation of pagan doctrines, we may reasonably expect to find pagan rites also preserved; and a knowledge of their tenets will therefore help us to understand the principles and practice of Ancient Paganism.

As the *form* of symbolism which is exoteric cannot affect the esoteric doctrines, we find that secret pagan sects adopt the outward form of the prevailing religion of the time; thus Gnosticism was clothed in a semi-Christian garb, and similarly the Sufis, Druzes and Nuseiríyeh make use of Mohammedanism as a cloak for their no less pagan doctrines.

The Nuseiríyeh worship a mystic triad consisting of Alí ibn Abi Táleb, Mohammed and Selmán el Farsi: the first is called the Meaning, the second the Chamberlain, and the third the Door.

From these proceed five other beings called Monads, to whom names, also borrowed from the companions and supporters of Ali, are given. These exercise the functions of creation and order.

The Devil is also a triune being, the three immediate opponents of Ali being taken as representatives of his avatars.

Their esoteric doctrine teaches that these personages only typify the heavenly bodies: thus Ali is the sky, Mohammed the sun, and Selman el Farsi the moon, while the five monads represent the planets known to the ancients, and their functions exactly correspond with those of the heathen gods whose names the planets bear.

The Nuṣeiríyeh believe in the transmigration of souls; they were originally stars, but fell through disbelief; those therefore who act well in this life will be restored to their celestial rank, while the bad will pass through successive stages of degradation in a future life.

Amongst other curious observances, they commence their prayers by the distribution of branches of olive or fragrant herbs to all the congregation, who on the repetition of a certain prayer place them solemnly to their noses. This is undoubtedly the rite mentioned by Ezekiel as appertaining to Syrian sun-worship: "Is it a light thing that they commit the abominations which they commit here? For they have filled the land with violence and have returned to provoke me to anger; and, *lo, they put the branch to their nose.*" Ezek. viii. 17.

They also make use of libations of wine, considering this as a symbol of the sun from its brightness and revivifying effects. Our own social practice of "passing the bottle the way the sun goes round," is a relic of the same Pagan superstition.

The Nuseiríyeh number about 5000 in Syria, and inhabit principally the districts around Laodicea and the mountains north of Aleppo.

The author then gave a slight sketch of the Sabæan creed, and concluded by repeating his belief that the principles of what may be termed astronomical worship lie at the root of all Pagan systems; and that in the tenets and philosophies of these Eastern sects will be found much that will assist in the

interpretation of the ancient mythologies and throw considerable light upon the nature of the Greek and Roman rituals.

Mr GEORGE WILLIAMS enquired whether Mr Palmer could give any information about the Metawaleh, and whether their tenets in any way resembled those of the Nuseirîyeh.

Mr PALMER replied that the Metawaleh were simply a sect of the Sheans, but that there was a slight connexion in that they held Ali in peculiar respect.

Dr CAMPION enquired whether the Nuseirîyeh practised sacrificial rites.

Mr PALMER replied that they sacrificed sheep and offered libations of wine; and that not later than 300 years ago (on the authority of Arab historians) they offered human sacrifices.

Nov. 14, 1870.

The PRESIDENT (PROFESSOR CAYLEY) in the Chair.

Communications made to the Society:

(1) *On a pipe in the chalk at Alum Bay. By* Professor LIVEING.

The peculiarity of this pipe is that it has a horizontal or nearly horizontal direction. It is seen at the S.E. corner of Alum Bay, where the surface of the chalk has been exposed by the washing away of the abutting clays, and presents no uncommon features except its direction; it is lined pretty equally all round with a layer of weathered chalk having a banded appearance from infiltration of iron, and is filled in the usual way with loose material, including some large flints such as are strewn over the surface of the chalk at its junction with the clays above. It is hardly possible to resist the conclusion that this pipe was formed before the chalk assumed its present highly-inclined position, for if it were merely a bent arm proceeding

from a pipe passing downwards from the present surface it must have left its trace above. If this be so, it will follow that the chalk of the Isle of Wight must have been sub-aerial at a time previous to the elevation of the Weald area; at least, if the theory of the formation of pipes by the percolation of water be accepted.

Professor Liveing made some remarks also on the shattered state of the flints at the same locality. As the loose flints on the eroded surface of the chalk which are imbedded in the superincumbent clay are shattered equally with those which retain their original position in the chalk, he inferred that it was probable that the shattering was not due to the great movement by which the chalk was placed on end, since although it is conceivable that the shock of such a movement communicated as a vibration through an unyielding mass like chalk may have shattered the flints which opposed a resistance to such a vibration, it is difficult to suppose that so yielding a matrix as clay could forcibly impress on flints such a vibration as to crack them through and through in every direction. He thought that either some other cause of the shattering must be sought, or that it must have taken place at some anterior period before the flints were weathered out of the chalk, and therefore before the deposition of the clays above.

(2) *On phenomena connected with denudation observed in the so-called Coprolite Pits near Haslingfield. By* Mr O. Fisher.

This paper contained an extension to the neighbourhood of Cambridge of observations heretofore made by the author, and described in former papers in the Journal of the Geological Society and in the *Geological Magazine*. They relate to the condition of the upper portion of the sections as seen as well in the coprolite pits as elsewhere. The upper three or four feet consist of travelled material, which in the cases described

consisted of portions of the coprolite bed transported in an unscattered condition from their original positions, shewing that some agency must have acted upon them to push them laterally over the surface, different from that of running water, whether in the form of rivers or of rain. And since it is evident that the same agent which has moved the superficial beds must be that which was engaged in the work of denudation, it was argued that rain and rivers have not been the sole influences to which the configuration of the landscape is due.

The previously published papers of the author on this subject point to land-ice as the denuding agent, and give reasons for supposing that the climate may have been sufficiently rigorous for such a condition about 100,000 years ago.

Mr Bonney expressed an opinion that the principal features of the district might more easily be accounted for by the action of lateral streams, aided by rain, at a period when the rainfall was greater than at present, and that the contortions and disturbances of the bed would better be explained by the grounding of small bergs floated off from an ice-foot than by the pressure of a glacier passing down the valley. He also thought it improbable that the valley had been occupied by a glacier since the Boulder clay period.

Mr Fisher replied that he could not interpret the phenomena as indicative of other than the action of an ice-sheet such as now enveloped parts of Greenland.

With reference to Professor Liveing's communication, Mr Fisher thought that the formation of the pipe at Alum Bay might be referred to the period of the Thanet Sands, which are unrepresented in the Isle of Wight, when denudation would naturally be taking place in that locality.

November 28, 1870.

The PRESIDENT (PROFESSOR CAYLEY) in the Chair.

Communications to the Society:

(1) *On a model of an electro-motive machine.*

(2) *On a model for transferring rotatory motion to a distance by means of a single wire.*

(3) *On a method of describing ellipses and* (4) *of drawing in perspective.*

(5) *On a steam-ship for conveying trains from Dover to Calais. By* J. C. W. ELLIS, M.A. *Sidney.*

The following are abstracts of Mr Ellis's explanations:

i. Electro-motive machine.

Consisting of three springs acted upon by electro-magnets successively. The springs, or elastic levers, are easily bent by the at first feeble magnetic influence, and as they approach the magnets resist with greater force but are overcome by the increased power of the magnets, and so the effect of the magnets is equalized and prolonged.

ii. Rotation conveyed to a distance by means of a single wire.

The original motion may be either one of rotation or a rectilinear backwards and forwards motion. Upright posts revolving about hinges at the foot carry the wire, which may be very fine and light by using leverage. The wire may be turned through any angle by applying a stay-wire to the post. The wire is finally applied to a wheel carrying a rachet, which drives a heavy fly-wheel. The rachet-wheel is drawn back by a spring. The advantage of this method consists simply in the ease with which it may be put up, its cheapness, and from its requiring no alteration or oiling. It is suitable for conveying power from a water-wheel to a farm for churning, gorse and chaff cutting, &c.

iii. A method of drawing ellipses.

A square frame hinged at the corners has a number of parallel strings, on these are arranged a number of concentric circles marked on the strings by beads or paint. When the square is drawn into the form of an oblong the circles become ellipses, and the two elastic strings stretched from corner to corner are the diameters, the equi-conjugate axes being parallel to the sides of the frame. This is manifest from the equation $x^2+y^2=a^2$ in the square remaining as $x^2+y^2=a^2$ in the rhombus. If ϕ be the angle of the rhombus, $\sec\frac{\phi}{2}=e$ gives the eccentricity. Or the portions of the elastic strings forming the diameters might be measured to suit the particular ellipse required. The frame can then be placed on a piece of paper and the ellipse dotted off between the parallel threads. Properties of ellipses and other curves may thus be compared with similar properties referred to rectangular co-ordinates.

iv. Method of drawing in perspective.

ABCD is a drawing frame.

The base *CD* is bisected in *E*. *EFG* is perpendicular to *CD* on the plane of the frame and projecting below the frame. *EG* is the distance of the eye. *GF* the height of the eye. If the plan of any building be drawn to scale at its true angle to and distance from *CD*, any point of it, *K*, is thrown into perspective thus: an elastic thread extends from *F* to *G*; it is stretched to *K*; let *GK* meet *CD* in *M*, and let *MS* perpendicular to *CD* meet *KF* in *S*; *S* is the perspective of *K*. The corresponding height of the *KT* is found by producing *MS* to meet *TF* in *Z*. This operation is facilitated by drawing on paper ruled parallel to a side of the frame perpendicular to *CD*.

v. Steam-ship to convey trains from Dover to Calais.

Two tubes of wood (or iron) 200 metres long and 4 metres diameter give carrying power of tubes when completely submerged = 5000 tons. Two parallel lines of rails to take trains,

and closed by water-tight sliding-doors. Engines, central paddle-wheel, refreshment rooms, &c. between lines of rail. Diameter of wheel 14 metres. Height of deck 6 metres. Weight of vessel 1200 tons; weight of engines (of 500-horse-power) + weight of coal and stores 200, + weight of train of twenty carriages 200 tons. Total, 2100 tons. Advantages—great speed, no oscillation. Tubes in compartments, and supplied with pumping apparatus connected with engine.

The PRESIDENT indicated a simple method of drawing an ellipse which he was in the habit of using himself.

On the Aurora Borealis. By JAMES STUART, M.A. *Trinity.*

There are several different kinds of Auroras, arches, bands, converging lines, general luminousness of the sky, &c. In some instances the convergence of the bands is due to perspective. The year of maximum aurora occurs every ten years, and at the same time as that of maximum sun spots, and of maximum perturbations of magnetism. Auroras in the northern and southern hemispheres are frequently simultaneous. Before an Aurora in the northern hemisphere and during the first part of the display the magnetic needle is deflected to the west; during the Aurora it makes frequent and violent excursions and then is deflected toward the east. In the southern hemisphere it is deflected also but in exactly the opposite way. This may be accounted for by currents running in the earth from the Poles to the Equator. De la Rue supposed these to be caused by electricity conveyed upwards at the Equator by evaporation, and thence to the Poles by the winds in the upper air, whence it was discharged into the earth at the Poles. The difficulty of such a thing is the nonconducting nature of the air. If a cloud of some interplanetary dust or medium of some kind were to come near the earth, the lower parts of this getting mingled with

the earth's atmosphere would acquire a velocity of rotation with the earth; electric currents would thus be generated owing to different parts of this medium cutting the earth's magnetic lines of force at different angular velocities; and the currents thus generated would be of the direction required to account for the perturbations of the magnetic needle. Such currents would also produce earth currents. The notion of meteoric haze being that which causes the Aurora may be connected with the fact that of 19 displays of Aurora selected from upwards of 250 during 13 years as being very brilliant, five occur on or about the 17th of November; and on ten of the 13 years there was an Auroral display at that time. The spectrum of the Aurora coincides with that of the zodiacal light and with that observed in a sky filled with luminous haze. Of Auroras observed at Dunse from 1840 to 1850 seventy-five were connected with stratification of cirrus clouds, the stratification of such clouds being parallel to the magnetic meridian while they moved slowly from S.W. to N.E. In those Auroras which have a "corona" it is in the prolongation of the direction of the magnetic dip. During an Auroral display the line of the dip seems generally to become more vertical.

Professor CHALLIS corroborated the account of the cloud coming and going, which on one occasion had been seen by himself at Cambridge, and Sir John Herschel at Collingwood. He had calculated the height of an Auroral arch at 175 miles. The streamers, he believes, go up hundreds of miles. The other phenomena mentioned by Mr Stuart about 175. The apparent convergence of the streamers is only an effect of perspective. He considered the streamers magnetic, and the Aurora produced by transverse streams. That there were two kinds of Auroras, one local, the other due to extraneous action. The Astronomer Royal had found that only the latter corresponded with disturbance of the needle. Auroras are said to be very common at Behring's Straits.

Mr POTTER said he remembered some brilliant displays not in a ten-year period, especially one of 1833; and he made some remarks in corroboration of Professor Challis' statements, and objected to the "dust" theory.

Mr STUART mentioned that the spectrum of the Aurora had been observed and shewn the same line between *D* and *E*, as had the zodiacal light; he briefly replied to Mr Potter.

Professor SELWYN exhibited some heliographs of the sun, extending over a period of 12 days, ending Sept. 30, all which had been very favourable for photography, at the time of the sun's maximum. They shewed a very remarkable number of spots. Another series, taken from Aug. 22—29, shewed also a large outbreak of spots. He called attention to the fact that in the northern and southern hemispheres, tornadoes went as the deflections in the magnetic needles, mentioned by Mr Stuart, viz. in opposite directions; and so did the sun's spots.

February 13, 1871.

The PRESIDENT (PROFESSOR CAYLEY) in the Chair.

Communication to the Society:

On the Operations of the Great Trigonometrical Survey of India in connexion with Geodesy. By Col. J. T. WALKER, R.E., F.R.S., *Superintendent of the Survey.*

[*Abstract.*]

After pointing out that there are three stages in the operations of a scientific national survey, namely, the trigonometrical or geodetic basis, the topographical delineation of the ground, and the construction of the maps, Col. Walker observed that the first stage has frequently been ignored, and that during the last century this was the case in India, surveys being carried

on without any basis of operation, and afterwards compiled into a general map in which the positions of a few of the chief towns in each province were determined by astronomical observations. Such determinations are always of questionable accuracy as the basis of an exact survey, even in the present state of science; and a century ago the tables of the places of the heavenly bodies were far less accurate than they are now, and frequently caused gross errors of position. Towards the close of the last century Major Lambton projected a "Mathematical and Geographical Survey" of southern India to determine by triangulation from measured base lines the positions of a number of permanent geographical marks to be afterwards the basis of a general survey of the Peninsula; but he pointed out that before the latitudes and longitudes of these marks could be correctly computed from the data furnished by the triangulation, the figure of the earth should be known with accuracy, and he suggested that his operations should be carried out in such a manner as to answer the requirements of a geodetic as well as of a geographical survey. His proposals were approved of by the Government, and the operations which he carried out were the commencement of the Great Trigonometrical Survey of India.

Col. Walker shewed that the combination of objects in a triangulation which is intended to serve the purposes of a geodetic as well as of a geographical survey necessarily introduces a very high order of accuracy into the operations, and that the Indian survey has derived much advantage from this circumstance. He described the nature of the operations of the survey, the manner in which they are conducted, and the instruments which are respectively used in making the linear and the angular measurements. The probable errors of the base lines are shewn to be $= \pm 2{\cdot}6$ millionth parts of the length measured, corresponding to a probable error of 108 feet in the length of the polar axis of the earth. The probable errors

of the angles measured with the great theodolites range from $\pm''\cdot2$ to $\pm''\cdot5$. The probable errors of the trigonometrically deduced ratios of the sides of the triangles are functions of those of the angles and of the geometric conditions of the triangulation. When the number of triangles is small the probable errors of the trigonometrical ratios are less than those of the ratios of the base lines, but in certain representative chains of triangles, averaging 575 miles in length and composed of a large number of triangles, the probable errors of the trigonometrical ratios of the base lines at their extremities are about three times those of the linear ratios. Thus the trigonometrical and the linear operations are fairly on a par with each other as regards accuracy.

Col. Walker then proceeded to give an account of the work which has been completed up to the present time, and shewed what remains to be done to finish the programme of operations. He pointed out that the present desideratum in geodesy is not so much a better determination of the mean figure of the earth, as of the variable figure at different parts of the earth's surface; and he stated that the operations of the Indian survey will, when supplemented by appropriate astronomical operations and differential determinations of longitude by the electric telegraph, furnish a number of meridional arcs and arcs of parallel which will be of great geodetic value.

The paper closed with an exposition of the methods which have been introduced by the author for the final reduction of the whole of the triangulation, so as to render all the parts consistent and harmonious.

Mr Ellis asked if there were any signs of the earth's expansion or contraction during the measuring of a base line.

Col. Walker said it could not be detected while an operation was going on. However, after an earthquake in Eastern Cachar the officer in charge asserted that the distance of some of the stations had been altered, but it was not certain whether

this was the case. To discover an alteration there would be need to remeasure the base line after an interval of time had elapsed.

Professor CHALLIS said it had been stated that there was an abnormal deviation of the plumb-line south of the Himalayas, and asked whether this had been observed on the north also, where, according to Col. Walker, some surveys had recently been made.

Col. WALKER said the surveys on the north side were too rough to be of value for geodetic purposes. On the south side of the pendulum observation shewed a deficiency of density as the hills were approached, and an increase on proceeding southwards towards the sea.

Some conversation followed, in which Prof. Adams, Prof. Miller, and Col. Walker took part.

February 27, 1871.

The PRESIDENT (PROFESSOR CAYLEY) in the Chair.

Communication to the Society.

On Observations made at San Antonio on the Total Solar Eclipse of 22 *Dec.* 1870. *By* W. H. H. HUDSON, M.A. *St John's.*

I propose to lay before the Cambridge Philosophical Society some account of the recent English expedition to observe the Total Eclipse of the Sun, which occurred on the 22nd of Dec. 1870.

This is not the place to describe the difficulties which had to be surmounted before the expedition could start at all—how some one had blundered and sent in the application to the wrong department—the natural rebuff received from a government subordinate—the unphilosophical huff thereat—the sus-

pension of all preparations—the discovery of the right department—the sudden hurried arrangements: there are those here who are better able to inform you on these points than I. But suffice it to say that, in spite of all indecision and bungling, two expeditions left these shores; of which one went overland to Sicily—strange to say, these were shipwrecked; and the other went by sea to Spain and Africa, having weathered what I may call the Professor's storm on the way.

The members of the latter expedition mustered on the evening of the 6th Dec. on board H.M.S. Urgent, Captain Henderson, to the number of about 35; there they were divided into three detachments, each under a separate leader. One under Dr. Huggins went to Oran in Algeria, another under Captain Parsons went to Gibraltar.

The third party, to which I had the honour to belong, was under the guidance of Father Perry of Stonyhurst, and met with better success: we landed at Cadiz, separating there from our companions on the ship. We were thirteen in number and were distributed to our several duties thus: four were to observe with the spectroscope, four with various polariscopes, four to sketch, and one to keep the time and make general observations.

The English expedition was not the only one in the neighbourhood of Cadiz. The Americans were a very strong party at Jerez (or Xeres), and Lord Lindsay, with great public spirit, had equipped a complete party, who were stationed at La Maria Luisa, about five miles west of Jerez. The Spaniards, who had a very fine observatory at San Fernando, near Cadiz, and to whose courtesy we were greatly indebted, sent a party to San Lucar, a town at the mouth of the Guadalquiver, about ten miles north-west of Lord Lindsay's position. Our temporary observatory was at San Antonio, intermediate to Puerto de Santa Maria and Jerez, about three miles from the former and five or six miles from either Lord Lindsay or the Americans.

Description of Instrument. The instrument used was a

refracting telescope by Dollond. The breadth of the object-glass was $3\frac{7}{8}$ inches; the focal length about 4 feet; the eye-piece was a negative one; the magnifying power 40.

There was a diaphragm in the eye-piece of the shape of a long parallelogram between the field and the eye-lenses. Outside the eye-lens was a double refracting prism of Iceland Spar, which caused such a separation of the images that when it was a maximum the short sides of the parallelogram were in the same straight line and the adjacent long sides just overlapped. This was the position in which it was used. The cap containing the diaphragm was furnished with a touch-mark consisting of a projecting spoke. When in adjustment as above described this mark was parallel to the short side of the parallelogram composing the diaphragm, and marked the plane of polarization of the image remote from it when the whole apparatus was turned round so that the difference of intensity was greatest.

A cardboard tube lined with black velvet was fixed at the object-end of the telescope, projecting 14 inches from the object-glass to prevent reflexion from the interior of the tube.

Experiments previous to leaving England: None.

The instrument was kindly lent by the Master and Fellows of St John's College, to whom my thanks and those of the Expedition are due. It was borrowed by the advice of Professors Stokes and Adams, the former of whom inspected it for the purpose, the latter recommended it from his previous acquaintance with it. It was only decided to ask permission to borrow it just in time to have cases made to convey it and its stand. It was therefore obliged to be immediately dismounted and could not be used. The eye-piece was arranged by Mr Ladd in accordance with the instructions of the Organising Committee, and was put into my hands in the railway carriage proceeding from Waterloo Station to Portsmouth on my way to join the Urgent. Consequently there were no experiments before leaving England with the instruments used.

Experiments at San Antonio.

On the Friday (the 16th) preceding the eclipse the instruments were fitted up at San Antenio, three miles or so from Puerto de Santa Maria; in this operation Lord Lindsay kindly afforded most valuable assistance. On that and every subsequent day till that of the eclipse, Thursday the 22nd, I was employed practising the manipulations of the telescope and observing the light from the sky, the clouds, the moon and various terrestrial objects for polarization.

At first the light from almost every object seemed to be polarized: this was accounted for by the want of perfect blackness in the tube of the telescope, and supposed to arise from reflexion in the interior. To correct this a projecting nozzle lined with black paper was first tried but found insufficient. Black velvet was next had recourse to and apparently with success.

On the night of the 17th or rather the morning of the 18th I was observing the moon and noticed that the light appeared to be polarized. Not expecting to have been able to detect this polarization of the light reflected from the moon I called out to Mr Moulton who was observing at the same time, and he had also seen bands across the moon with his instrument.

On repeating the observation I detected no polarization and Mr Moulton saw no bands. We were led to account for this by the presence of thin clouds: when the clouds were thick and when the sky was quite clear I could detect no difference of intensity on turning the analyser. When there were thin clouds, and even when no clouds were visible to the eye, but when their existence was rendered probable by the neighbourhood of visible clouds, the polarization became manifest.

I subsequently repeated these experiments near the sun at noon when the sun was so clear that I had to use a dark glass; there were however light clouds about. I observed the sky in the immediate neighbourhood of the sun, approximately within

a radius of its limb, and at several parts I found the light polarized, the plane of polarization being always radial. Some of these observations Mr Ladd verified at the time.

I detected no polarization when thick clouds were on the sun, either on the sun or in its immediate neighbourhood; but at a greater distance, 2 or 3 radii from the limb, I found polarization, and again the plane was normal to the limb.

Observations during the Eclipse.

I repeated these observations during the progress of the eclipse after first contact and before totality. At $11^h. 8^m. 10^s.$ (11. 15. 30 G. M. T., time taken by Capt. Toynbee) just as the moon's limb was in contact with a spot (the second of two spots near together) I found polarization on the moon's limb: immediately after contact I found the same again. Later at $11^h. 32^m.$ (11. 39. 20 G. M. T.) the polarization was more decidedly marked, and I determined its plane to be at an angle of 45° to the horizon.

Again at 3^m before totality, when thick clouds came on, I found no polarization visible.

Observations during and about Totality.

1°. I watched for the first appearance of the corona in the East (apparently left) limb of the moon: this I saw at $12^h. 7^m. 40^s.$ (12. 15 G. M. T.).

2°. I tested the corona for polarization before totality and found none.

Note. This observation was not completed before totality; in the course of it I heard from behind me a shout of "The Corona!"

3°. I tested the moon's surface for atmospheric polarization. I found that it was visible and that its plane was the same as that I had determined before totality.

4°. In the course of this observation I noticed that the brightness of the moon was very considerably more than I expected: not so much less than before totality. I agree in

the description I had seen of its "green velvety" appearance. The green was like that of the olives that were commonly on the dinner-table in that neighbourhood, but greener and less brown. The clouds drifting across the moon were perceptible, and the mountains in the moon were not.

5°. I examined the corona for polarization near the apparent upper surface of the moon; I found that it was apparently polarized and that the diaphragm was vertical when the two images were of equal intensity. I did not determine its plane, but it must have been inclined at 45° to the horizon and therefore neither radial nor tangential.

6°. Time was nearly up. I took my eye from the telescope and for a few seconds saw the general appearance of the corona and prominences. I distinctly noticed the quadrilateral shape of the corona and that the sides of the quadrilateral were roughly horizontal, and vertical. The greatest extension was to the N.W. and was about $\frac{3}{4}$ of the moon's diameter.

7°. I returned to the telescope to watch for the final disappearance of the corona after totality. This occurred at $12^h. 10 . 30^s$. (12. 17. 50. G. M. T.).

I had seen the corona for 2 . 50^s, about $\frac{3}{4}$ of a minute longer than the totality.

8°. Immediately after totality I hastily made a sketch of the impression scarcely gone from my eye of what I saw without the telescope; this was very rudely and almost automatically done, but agrees very closely with Mr Browne's sketch.

Mr Browne was standing by my side to make the sketch: he did not see mine nor I his till both were completed.

Conclusions.

I draw the following conclusions from the observations made during the eclipse and during the previous week.

1°. All observations of polarization made with telescopes that have not been previously tested for blackness inside, are utterly untrustworthy.

2°. That the appearances of polarization are presented when light shines through a thin cloud, whether the said cloud be visible or invisible.

3°. That this effect is not produced by clouds above a certain thickness. (N.B. I estimate the thickness of a cloud by its darkness: the quantity of light it absorbs.)

4°. I believe that the polarization of the corona which I detected was simply due to the intervening atmosphere.

I am not perfectly certain of the absolute success of the black velvet arrangement. I think that there may have been some slight reflexion in the tube; had the light been stronger this might have had a perceptible effect; as the light was so much cut off by clouds, I believe that it was practically successful.

I believe the light was quite sufficient and the instrument sufficiently sensitive to enable me to speak with great confidence when I determined that there was polarization, and to prove that when I failed to detect it it must have been very small. My determination of plane is certainly within 5° and probably closer.

So much for my own observations. I now add a few words on the general results of the expeditions as far as they are yet gathered up.

1. From the spectroscope we learn that the light of the corona contains the hydrogen lines and also the unknown line 1474.

2. The shape of the corona is not circular, but roughly quadrilateral; it is broken by indentations and by a conspicuous V-shaped gap towards the S.E. corner, seen not only at three stations in Spain but also in Sicily.

3. The inner brighter corona, which is supposed to have a defined outer boundary separating it from the outer corona or glory, is probably an optical or subjective effect.

4. The question of the polarization of the corona still re-

mains open; there is not a sufficient number of accordant results free from suspicion, either on account of unfavourable sky or inaccuracy of instruments.

Professor W. G. ADAMS then exhibited photographs by the oxyhydrogen lime-light.

The photographs shewed the corona as seen from Sicily, having been taken at Syracuse by Mr Brothers. In exhibiting the photographs, Professor Adams called attention to various points of interest in them.

There were also passed round copies of the American photograph taken at Jerez in Spain, of Mr Brothers' photograph, also an enlarged copy of the latter and a reduced copy of the former, so as to bring them to the same scale, mounted so as to shew the resemblance between the two photographs taken at a distance of 1200 miles apart.

There were exhibited on the screen some maps to shew the geographical position of the observing stations, and, side by side, drawings made by Mr Hudson and by Mr Browne, of Wadham College, Oxford, from their sketches of the corona.

Mr CLIFFORD, who was with the Sicilian Expedition, described its comparative failure, but said that some observations made when the disc of the moon was comparatively free from clouds confirmed Mr Hudson's to a great extent.

Mr MOULTON, who was stationed at San Lucar, was of opinion that the polarization seen was attributable to defective instruments. Experiments had proved that polarization was not observed when the polarizer was placed, not at the eye-end, but at the other end of the instrument. The instrument had been thus used by the American observers, and they had never been able to detect radial polarization. His own experience, however, would not throw much light on the subject, owing to the tantalization of the clouds at the time of his observations, and when the eclipse was approaching totality. He could

mainly corroborate Mr Hudson; the observation of the V gap, however, was wanting.

He then read a very interesting communication from Father PERRY, summing up the results of the spectroscope observations at the various stations.

Starting outwards from the photosphere, Prof. Young and Mr Pye had proved the existence of a thin absorption band covering the photosphere, and ordinarily giving rise to the dark lines: this, as soon as the photosphere was eclipsed, burst forth as an innumerable mass of bright lines, to last only for a second or two, and then itself to be eclipsed: passing through the chromosphere, we come to an outer layer of cooler hydrogen, whose tale has been told by the spectroscope of Mr Abbay, and then at last to a hollow shell of green vapour, lighter than hydrogen, whose spectrum is the bright green line 1474, extending into space far beyond any measurable limits, and which will probably enter largely into all future theories of the ether of space.

Mr CLIFFORD added a few words, maintaining that the disturbing causes did not apply to the instruments which he was using.

March 13, 1871.

The PRESIDENT (PROFESSOR CAYLEY) in the Chair.

New Fellows elected:

H. M. TAYLOR, M.A.
M. R. PRYOR, B.A. } *Trinity College.*
J. W. L. GLAISHER, B.A.
H. M. GWATKIN, M.A., *St John's College.*
Rev. B. WALKER, M.A., *Corpus Christi College.*

Communications made to the Society:

On the Attraction of an infinitely thin Shell bounded by two similar and similarly situated concentric ellipsoids on an external point. By Professor Adams.

No problem has more engaged the attention of mathematicians, or has received a greater variety of elegant solutions, than that of the determination of the attraction of a homogeneous ellipsoid on an external point.

Poisson's solution, which was presented to the Academy of Sciences in 1833, is founded on the decomposition of the ellipsoid into infinitely thin shells bounded by similar surfaces. By a theorem of Newton's, it is known that such a shell exerts no attraction on an internal point, and Poisson proves that its attraction on an external point is in the direction of the axis of the cone which envelopes the shell and has the attracted point for vertex, and that the intensity of the force can be expressed in a finite form, as a function of the co-ordinates of the attracted point.

In 1834, Steiner gave, in the 12th volume of Crelle's *Journal*, a very elegant geometrical proof of Poisson's theorem respecting the *direction* of the attraction of a shell on an external point. He shows that if the shell be supposed to be divided into pairs of opposite elements with respect to the point in which the axis of the enveloping cone meets the plane of contact, then the resultant of the attraction of each pair of such elements acts in the direction of the axis of the cone, and consequently the attraction of the whole shell acts in the same direction.

About three years later, M. Chasles showed that Poisson's solution might be greatly simplified by the consideration that the axis of the enveloping cone is identical with the normal to the ellipsoid which passes through the attracted point and is confocal with the exterior surface of the shell.

This mode of enunciating the direction of the attraction has the advantage of making known the level surfaces with respect to the attraction of the shell on external points.

In 1838, M. Chasles presented to the Academy of Sciences a very simple and elegant investigation, in which he arrives at Poisson's results respecting the attraction of a shell on an external point, by a purely synthetical method.

M. Chasles' method is founded on Ivory's well-known property of corresponding points on two confocal ellipsoids, and on some elementary propositions in the theory of the Potential.

Struck by the simplicity and beauty of Steiner's method of finding the *direction* of the attraction of a shell on an external point, the author of the present paper was induced to think that by means of the same method of decomposing the shell into pairs of elements employed by Steiner, a correspondingly simple mode of determining the *intensity* of the attraction might probably be found. The author has been fortunate enough to succeed in realizing this idea, and the result is the method contained in the first part of the present paper.

This method is throughout quite elementary. It requires the knowledge of only the most simple properties of ellipsoids, including Ivory's well-known property respecting corresponding points on two confocal ellipsoids.

The proof of the theorem respecting the direction of the attraction differs from that given by Steiner, and harmonizes better with the method employed for determining the intensity of the force. No use is made in this method of the properties of the Potential.

The second part of the present paper is devoted to what the author considers to be an improvement on M. Chasles' method of determining the attraction of a shell on an external point. Its novelty consists in the mode in which the *intensity* of the attraction of the shell is found. M. Chasles first compares the attractions of two confocal shells on the same external point

He then takes the outer surface of one of these shells to pass through the attracted point, and having found the attraction of this shell by a method applicable to this particular case, he deduces from it the attraction of the general confocal shell. Now it may be remarked on this that the method of finding the attraction of the shell contiguous to the attracted point does not seem free from objection, and also that it may be doubted whether it is legitimate to include this limiting case under the general one without a special examination. If, in order to remove these objections, special considerations are introduced, the proof is thereby deprived of its simple and elementary character. Whether these criticisms on M. Chasles' method are well founded or not, the author thinks that mathematicians will not be displeased to see a direct determination of the attraction of a shell on an external point without the intervention of another shell whose outer surface passes through that point. In order to make the paper more complete, the author briefly shows how from the expression for the attraction of a shell, we may pass to the expression the integral of which gives the attraction of a homogeneous ellipsoid on an external point.

On a theory of the forms of floating leaves in certain plants. By W. P. Hiern.

Consider the curved margin of an undivided portion of a leaf which floats in a stream exposed to the resistance of the current; suppose that the power of growth is exerted equally at all points of the margin, tends to push the margin normally outwards so as to oppose rather than co-operate with the current, and is just balanced at the instant considered by the other mechanical forces which act on the margin; and further suppose that the margin remains as a flexible curve with tangential tension but not submitted to either normal strains or wrenching couples.

It then follows from merely mechanical reasons that the tangential tension is the same at all points, and that the form of the portion of the margin at the instant under consideration is determined by one of the following intrinsic equations:

$$\tan\left(\frac{s}{l} \cdot \cos \alpha\right) = \cos \alpha \cdot \tan \phi, \quad \text{or} \quad e^{\frac{2s}{l} \cdot \cot \beta} = \frac{\sin (\beta + \phi)}{\sin (\beta \sim \phi)},$$

according as the vigour of growth is more or less than sufficient to overpower the direct resistance of the current.

In these equations s represents the length of the arc of the margin measured from that point of it where its tangent is in the direction of the current to the point where the tangent is inclined to that direction at the angle ϕ; and l and α or β are quantities dependent only upon the proportional values of the tangential tension, the power of growth and the direct resistance of the current. The first equation when traced furnishes a series of separate ovals (but not ellipses) the longest diameters of which all lie on one straight line perpendicular to the direction of the current; the second equation furnishes a pair of catenary-like curves with their convexities opposed to each other, which become actual catenaries when the power of growth would just balance the direct resistance of the current. Parts only of these curves are applicable to the case of the portions of the leaf-margins according to the original hypothesis, and in no case are those parts of the curves applicable which correspond to points where ϕ lies between 180° and 360°.

After the leaf-margin ceases to be flexible, as for instance after the completion of its growth, the investigation can be extended to calculate the tangential tensions, the normal strains and the wrenching couples to which it is then submitted at the different points of the margin; and tolerably simple expressions are found for them.

The above equations are only suitable for those leaves in which the structure is pretty uniform in all directions, as in

plants either with very little structure or with highly reticular venation. For many monocotyledonous plants some modification is necessary in the mechanical hypothesis, and curves more elongated in the direction of the current are obtained.

Mr SEELEY asked whether there was any corresponding law to be discovered for leaves which were not floating but aerial: he thought it might be possible to refer all leaves to such laws. With regard to the influence of pressure on growth in leaves, so long as it did not override vital force in the plant, he thought it fell in with what was to be observed in animal growth, which required intermission of pressure, for continuous pressure would stop growth.

Mr HIERN said that in the case of aerial leaves the problem was far more difficult, seeing that the water neutralized gravity and the edges of the leaves were more flexible. He had considered that case a little, but preferred to discuss the simpler case first, with the hope of at some future time working out the more complex. He pointed out that there always was some little pressure on leaves from surface currents.

Dr PAGET asked whether Mr Hiern had tested his theory by some simple experiments?

Professor CAYLEY asked whether he had examined the effect on the leaves of the same plant in still and running water?

Mr HIERN said that the forms of leaves were apparently modified by circumstances, and that the experiments suggested would not be easy.

On the effect of exhaustion and inflation of the tympanum in deadening sounds, and on the test of loudness. By Mr MOON.

The author discussed the problem "Why, when the tympanal cavity is either exhausted or inflated, low tones are more affected than high tones?" Before doing this he considered the ordinary explanation of what is the test of the loudness of a

sound, viz. that its intensity is measured by the amplitude or the square of the amplitude of the vibrations by which it is produced, which he considered incomplete when waves of different lengths are compared. The conclusion at which he arrived was that, if two notes were sounded separately, differing by seven octaves, when the tympanal cavity was exhausted or inflated, the force of resistance to the motion of the aural nerve was the same in either case, but that the primary force (that which is lessened by the resistance) was 128 times as great in the one case as in the other; therefore the resistance in the one case might be sufficient to stop the motion of the nerve entirely, i.e. to suppress the note, while in the case of the higher note, the effect on the motion of the nerve, and therefore in the degree of distinctness with which the note is perceived, might be practically inappreciable.

May 1, 1871.

The PRESIDENT (PROFESSOR CAYLEY) in the Chair.

New Fellows elected:

Rev. G. HALE, M.A., *Sidney Sussex College.*
Rev. C. SMITH, B.A., *Sidney Sussex College.*
A. G. GREENHILL, B.A., *St John's College.*

Communications made to the Society:

On the Measurements of an Arc of the Meridian in Lapland. By I. TODHUNTER, M.A.

The object of this Memoir was to draw attention to the numerous errors which have been made, even by distinguished astronomers, in their accounts of the two measurements of an arc of the meridian in Lapland. A comparison of the original authorities on the subject at once detects these errors and supplies the necessary corrections.

Professor MILLER asked whether a zenith sector referred to by the author was the one used at the equator, some faults of which he pointed out; he also enquired whether observations had been made to determine the eccentricity of the quadrants used in the Lapland measurements.

Mr TODHUNTER said the sector was not the same; and that there was no clear information given on the other points.

Mr GODFRAY asked if the toises used in the observations had been compared.

Mr TODHUNTER replied in the affirmative. Some further conversation occurred in which the President (Prof. CAYLEY) and Mr ELLIS took part.

May 15, 1871.

The PRESIDENT (PROFESSOR CAYLEY) in the Chair.

The Treasurer (Dr CAMPION) gave a statement of the financial condition of the Society, which he pronounced to be satisfactory. A vote of thanks, proposed by Prof. BABINGTON and seconded by Prof. SELWYN, was heartily accorded.

Communications made to the Society:

(1) *On Dr Wiener's Model of a Cubic Surface with 27 lines; and on the Construction of a double-sixer. By* Prof. CAYLEY.

I call to mind that a cubic surface has upon it in general 27 lines which may be all of them real. We may out of the 27 lines (and that in 36 different ways) select 12 lines forming a "double-sixer," viz. denoting such a system of lines by

$$\begin{array}{cccccc} a_1, & a_2, & a_3, & a_4, & a_5, & a_6, \\ b_1, & b_2, & b_3, & b_4, & b_5, & b_6, \end{array}$$

then no two lines a meet each other, nor any two lines b, but each line a meets each line b, except that the two lines of a pair (a_1, b_1), (a_2, b_2),...(a_6, b_6) do *not* meet each other. And such

a system of twelve lines leads at once to the remaining 15 lines; viz. we have a line c_{12}, the intersection of the planes which contain the pairs of lines (a_1, b_2) and (a_2, b_1) respectively.

The model is formed of plaster, and is contained within a cube, the edge of which is = 18·2 inches; the lines a, b, c are coloured blue, yellow, and red respectively; the lines a_1, b_2, b_5 being at right angles to each other, in such wise that taking the origin at the centre of the cube, the axes parallel to the edges thereof, and the unit of length = 1·6 inches, the equations of these three lines are

$$\begin{array}{lll} a_1 & x=0, & y=0, \\ b_2 & x=0, & z=1, \\ b_5 & y=0, & z=-1. \end{array}$$

The model is a solid figure bounded by portions of the faces of the cube, and by a portion of the cubic surface, being a surface with three apertures, the collocation of which is not easily explained.

(2) *On the Tides in a rotating Globe covered by a Sea of constant depth at all points in the same latitude, and attracted by a Moon always in the plane of the equator, supposed either fixed or moving with uniform angular velocity; considered with reference to the tides as they are in nature, and the retardation of the earth's angular motion.*

(3) Also, *On the motion of imperfect fluid in a hollow sphere rotating about its centre under the action of impressed external periodic forces, considered with reference to the phenomena of Precession and Nutation. By* Mr Röhrs.

In the first of these papers it was shewn that by assuming the Moon to be in the equator always, her effect would be greater

than in nature; and by assuming that the sea was relatively at rest close to the bottom and stuck to it, so to speak, the greatest possible effect of roughness of the bottom was obtained. These data being assumed, the problem became a simple but rather lengthy application of Professor Stokes' equations of fluid motion; and the result of the investigation was to shew that the internal friction of the sea was too small to produce a sensible effect; the angle of lagging not being more than 2″ or $\frac{1}{100\,000}$ for an ocean of which the depth or latitude was supposed to vary so as to give a tidal range nearly on the scale of nature. The retardation due to this small value of λ would not be more than enough to occasion an increase in the length of the day of one second in *one hundred millions of years!* Practically none at all. Mr Röhrs admitted that close to shores and in narrow channels the retarding action of the sea would be greatly magnified, but he thought that this increased action would be more than counterbalanced by the entire absence of tidal retardation in the parts of the globe where no sea existed. Besides, the hypothesis of the sea "sticking to the bottom" gives an amount of retarding force due to the roughness of the sea-bed greater than what could be the case under any circumstances; hence the value of λ so obtained will be a superior limit. One second of retardation in 100,000,000 years would only make an error of 12 seconds in the date of an eclipse observed 2500 years ago; and as the error to be accounted for in eclipses then recorded is about an hour and a half, 450 times 12 seconds is required; therefore tidal retardation cannot, as some have supposed it might, account for this discrepancy.

The discussion of the second problem shews that if a globe, 4000 miles in diameter, composed of a thin crust and imperfect fluid interior, be made to rotate under the action of a force going through its phases in 27000 years, and if w and w' be the angular velocities of the globe at its crust, on the hypothesis of

the globe being solid throughout, and fluid respectively; and if $w = n \sin pt$, w' will be $= n' \sin pt - m' \cos pt$, where m' will be at least $\frac{1}{35}$th of n, $\therefore n' = n$ nearly, unless μ' the coefficient of fluid friction of the globe's interior be at least 150,000,000 times that of water; and for forces going their phases in 20 years μ', in order that m' may be not more than $\frac{n}{35}$, must be at least $\frac{27000}{20} \times$ this great value, or at least 200,000,000,000 the value of μ' in water. The application of these facts to the phenomena of Precession and Nutation is obvious, and points to a condition of the interior of the globe which is inconsistent with all notions of fluidity; since a deviation, not merely from the value, but from the force of w to the extent shewn above, would not escape observation and detection.

Mr Röhrs observed in conclusion, that he did not know how far, or if at all, he had been anticipated by other persons in the solution of these problems; as the problems present no difficulties of analysis, it was likely he had been, but he was not aware of that fact—his work was entirely his own. He had heard, however, that since his paper had been in the hands of the Society, that is, within the last year or more, Mr Stone, of Greenwich, had announced that tidal retardation was practically insensible in amount.

Professor Stokes thought that it would not be safe to assume (as had been done) the value of the constant which had been determined by himself, for in investigating that case the motion in the fluid had been supposed very slow, so that no eddies were formed, and fluid friction only acted. But in the problem of the tides, as the bed of the ocean would generally be rough, the formation of eddies would be an important element in the matter, and thus the resistance might be much greater than in the case contemplated above.

Mr O. Fisher mentioned that Archdeacon Pratt had replied

to Delaunay's criticisms in some letters published in *Nature;* and read an extract from the last of these. He asked whether it was true that the axis of the fluid nucleus would not be affected by the motion of the shell of the earth, and whether it would not drag upon the fluid interior.

Mr RÖHRS replied that it would drag.

Professor STOKES said that a hollow sphere with a *perfect* fluid within would pass over the fluid without dragging, whereas a very viscous fluid would follow that sphere; therefore it was entirely a question of degree.

Professor CAYLEY enquired whether Mr Röhrs had examined Delaunay's paper, in which he accounted for the difference between the observed and calculated time of ancient eclipses by a retardation leading to an alteration in the length of the day.

Professor STOKES made a few remarks on the mode of attempting the problem in the former of Mr Röhrs' communications.

May 29, 1871.

The PRESIDENT (PROFESSOR CAYLEY) in the Chair.

Fellow elected:

M. FOSTER, M.A., M.D., *Trinity*.

Honorary Members elected:

Prof. Sir BENJAMIN COLLINS BRODIE, M.A., F.R.S.
W. B. CARPENTER, M.D., F.R.S.
A. R. CLARKE, Capt. R.E., F.R.S.
Prof. T. HUXLEY, M.D., F.R.S.
Prof. BARTHOLOMEW PRICE, M.A., F.R.S.
WILLIAM SPOTTISWOODE, M.A., Treas. R.S.
Prof. F. W. A. ARGELANDER (Bonn).
Prof. A. CLEBSCH (Göttingen).
Prof. A. O. DES CLOISEAUX (Paris).
Prof. H. HELMHOLZ (Berlin).
Prof. F. WÖHLER (Göttingen).

Communications made to the Society:

(1) *On an illustration of the empirical theory of Vision.* *By* Mr COUTTS TROTTER.

Mr TROTTER gave an account of some experiments which he had made. With one eye he was shortsighted, the other had the ordinary range of vision. On using spectacles with one glass concave and the other plane, so as to bring the defective eye up to the normal range without altering that of the other. he found some difficulty in judging distances, &c. Thus when both his eyes had the same focal length, and were thus both perfect instruments, he did not see so accurately as when one of them was an imperfect instrument. Hence he contended that the result of his experiments supported the empirical theory of vision.

(2) *On a Table of the Logarithms of the first* 250 *Bernoulli Numbers.* *By* Mr GLAISHER.

Mr Glaisher's communication was not of a nature to be given in abstract.

(PART XIII.)

PROCEEDINGS

OF THE

Cambridge Philosophical Society.

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Cambridge:
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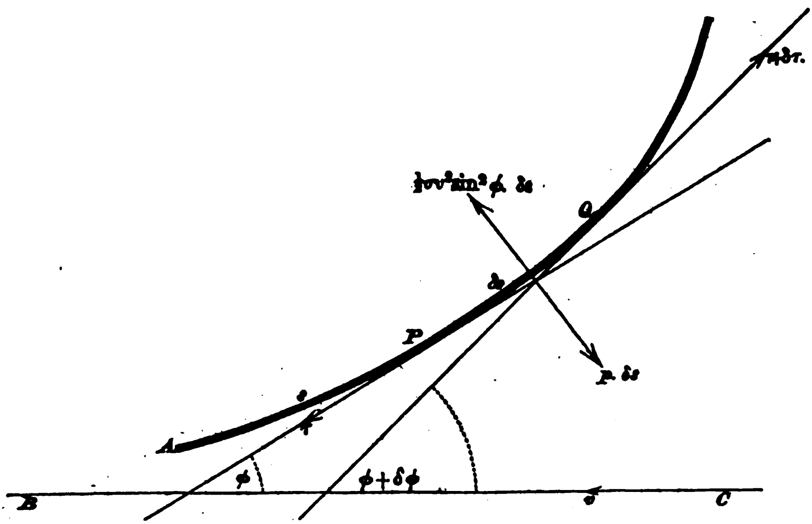
AT THE UNIVERSITY PRESS.

A Theory of the Forms of Floating Leaves in certain Plants. By W. P. Hiern, M.A.

[Read *March* 13, 1871.]

When a flat leaf with flexible margins grows steadily under favourable conditions and floats in running water, let it be supposed that during growth the action between two contiguous portions of an undivided part of the margin is entirely tangential and of the nature of tension, and not submitted either to normal strains or wrenching couples. Let it also be supposed that the vital power of growth as exhibited at the margin at any instant may be expressed mechanically at each point



by a normal pressure outwards p, which is constant for all points at the instant under consideration.

Thus if APQ be a portion of the curvilinear margin of a leaf exposed to the resistance of the current which is moving with velocity v in the direction CB, σ the density of the water, τ the tension at the point P, $\tau+\delta\tau$ the tension at the contiguous point Q, the arc $AP=s$, $AQ=s+\delta s$, ϕ the angle made by the tangent at P with BC, $\phi+\delta\phi$ that at Q; then, assuming the usual law of resistance due to the current, the element PQ when the power of growth is just balanced, will be in equilibrium under the following mechanical forces:

Tension τ at P along tangent at P in a direction remote from Q,

tension $\tau+\delta\tau$ at Q along tangent at Q in a direction remote from P,

resistance $\frac{1}{2}\sigma v^2\sin^2\phi\,.\,\delta s$ normally inwards,

pressure $p\,.\,\delta s$ normally outwards.

By resolving these forces first tangentially with respect to P and then normally, the following equations are obtained:

$$-\tau+(\tau+\delta\tau)\cos\delta\phi+\left(p-\frac{1}{2}\sigma v^2\sin^2\phi\right)\delta s\,.\sin\frac{\delta\phi}{2}=0,$$

$$(\tau+\delta\tau)\sin\delta\phi-\left(p-\frac{1}{2}\sigma v^2\sin^2\phi\right)\delta s\,.\cos\frac{\delta\phi}{2}=0;$$

and passing to the limit when $\delta\tau$, $\delta\phi$, δs are indefinitely diminished, it is readily seen that

$$\frac{d\tau}{ds}=0,$$

$$\frac{d\phi}{ds}=p-\frac{1}{2}\sigma v^2\sin^2\phi;$$

therefore τ is constant for all points, and

$$\frac{ds}{d\phi} = \frac{\tau}{p - \frac{1}{2}\sigma v^2 \sin^2\phi};$$

therefore $$\frac{p}{\tau}.s = \int \frac{d\phi}{1 - \frac{1}{2}.\frac{\sigma v^2}{p}\sin^2\phi} = \int \frac{d.\tan\phi}{1 + \left(1 - \frac{\sigma v^2}{2p}\right).\tan^2\phi}$$

In the integration indicated by the last expression, two cases arise according as p is greater or less than $\frac{1}{2}\sigma v^2$, that is, according as the power of growth is more or less than sufficient to balance the direct resistance of the current.

In the first case take α a subsidiary angle such that

$$\sin^2\alpha = \frac{\sigma v^2}{2p},$$

then $$\frac{p}{\tau}.s = \int \frac{d.\tan\phi}{1 + \cos^2\alpha.\tan^2\phi} = \frac{1}{\cos\alpha}.\tan^{-1}(\cos\alpha.\tan\phi);$$

no constant is required if s and ϕ vanish together.

Also if x, y be the rectangular coordinates of P, the axis of x being parallel to BC,

$$\frac{dx}{d\phi} = \frac{dx}{ds}.\frac{ds}{d\phi} = \cos\phi.\frac{\tau}{p(1 - \sin^2\alpha.\sin^2\phi)},$$

$$\therefore \frac{p}{\tau}.x = \int \frac{d.\sin\phi}{1 - \sin^2\alpha.\sin^2\phi} = \frac{1}{2\sin\alpha}\log\frac{1 + \sin\alpha.\sin\phi}{1 - \sin\alpha.\sin\phi};$$

so $$\frac{p}{\tau}.y = \int \frac{\sin\phi\, d\phi}{1 - \sin^2\alpha.\sin^2\phi} = -\int \frac{d.\cos\phi}{\cos^2\alpha + \sin^2\alpha.\cos^2\phi}$$

$$= \frac{-1}{\sin\alpha.\cos\alpha}.\tan^{-1}(\tan\alpha\cos\phi);$$

no constants are required if x vanishes when $\phi = 0$, and y when $\phi = \frac{\pi}{2}$.

Therefore on eliminating ϕ,

$$e^{\frac{p}{\tau}.\sin\alpha.x}+e^{-\frac{p}{\tau}.\sin\alpha.x}=\pm 2\sec\alpha.\cos\left(\sin\alpha\cos\alpha.\frac{p}{\tau}.y\right).$$

In the second case, that is, when p is less than $\frac{1}{2}\sigma v^2$, assume

$$\sin^2\beta=\frac{2p}{\sigma v^2},$$

then $$\frac{p}{\tau}.s=\int\frac{d.\tan\phi}{1-\cot^2\beta.\tan^2\phi}=\frac{\tan\beta}{2}.\log.\frac{\tan\beta+\tan\phi}{\tan\beta\sim\tan\phi}$$

$$=\frac{\tan\beta}{2}.\log.\frac{\sin(\beta+\phi)}{\sin(\beta\sim\phi)};$$

$$\frac{p}{\tau}.x=\int\frac{\cos\phi.d\phi}{1-\operatorname{cosec}^2\beta.\sin^2\phi}=\frac{\sin\beta}{2}.\log.\frac{\sin\beta+\sin\phi}{\sin\beta\sim\sin\phi}$$

$$=\frac{\sin\beta}{2}.\log.\frac{\tan\frac{\beta+\phi}{2}}{\tan\frac{\beta\sim\phi}{2}},$$

$$\frac{p}{\tau}.y=\int\frac{\sin\phi.d\phi}{1-\operatorname{cosec}^2\beta.\sin^2\phi}=-\int\frac{d.\cos\phi}{\operatorname{cosec}^2\beta.\cos^2\phi-\cot^2\beta}$$

$$=\tan^2\beta\int\frac{d.\cos\phi}{1-\sec^2\beta.\cos^2\phi}=\frac{\sin^2\beta}{2\cos\beta}.\log.\frac{\cos\phi+\cos\beta}{\cos\phi\sim\cos\beta}$$

$$=\frac{\sin^2\beta}{2\cos\beta}.\log.\left(\cot\frac{\beta+\phi}{2}.\cot\frac{\beta\sim\phi}{2}\right);$$

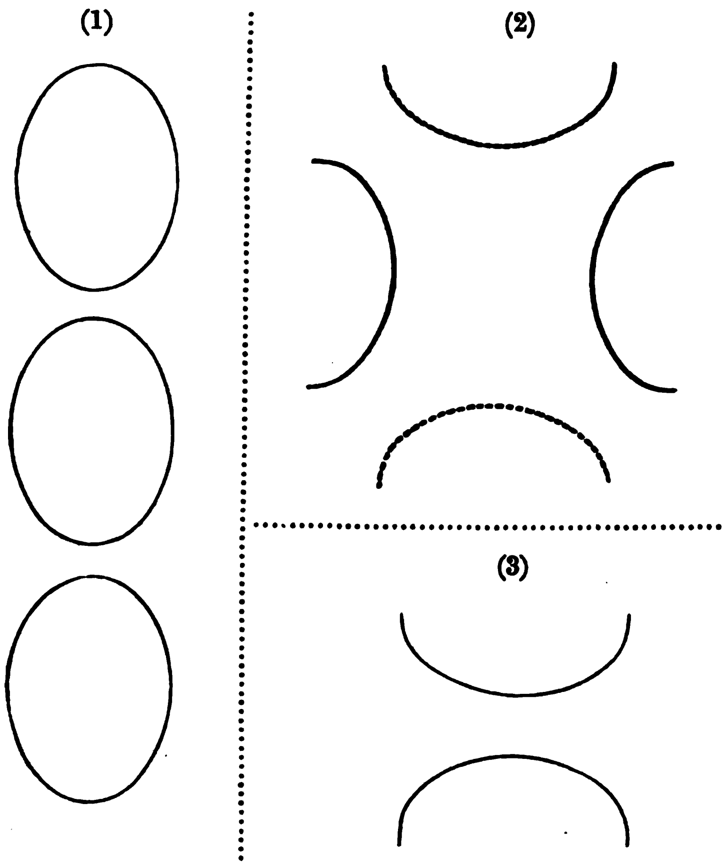
after eliminating ϕ, the rectangular equation is found to be

$$\tan\beta=\frac{\pm e^{\frac{\cos\beta}{\sin^2\beta}.\frac{p}{\tau}.y-\frac{1}{\sin\beta}.\frac{p}{\tau}.x}\pm e^{\frac{\cos\beta}{\sin^2\beta}.\frac{p}{\tau}.y+\frac{1}{\sin\beta}.\frac{p}{\tau}.x}}{\pm e^{\frac{2\cos\beta}{\sin^2\beta}.\frac{p}{\tau}.y}-1}$$

In the case when $p=\frac{1}{2}\sigma v^2$,

$$\frac{p}{\tau}.s=\int\sec^2\phi\,d\phi=\tan\phi,\qquad e^{\frac{p}{\tau}.x}+e^{-\frac{p}{\tau}.x}=\pm 2\frac{p}{\tau}.y.$$

The curves whose equations have thus been determined can be traced either from the intrinsic or rectangular equations; in the case when $p > \frac{1}{2}\sigma v^2$ a series of equal detached ovals is obtained whose longest diameters all lie on one straight line, perpendicular to BC; and as the curvature continually diminishes from the point where $\phi = 0$, to that where $\phi = \pm\frac{\pi}{2}$, the diameters parallel to BC are the least, and those perpendicular to BC the greatest. In the second case when $p < \frac{1}{2}\sigma v^2$ a



pair of equal catenary-like curves is obtained with their convexities opposed to each other, which in the last case, when $p = \frac{1}{2}\sigma v^2$, become actual catenaries.

Those parts of the curves obtained above which correspond to points from $\phi = 0$ to $\phi = \pi$ are alone applicable to the original hypothesis that the resistance due to the current opposes the power of growth; parts corresponding to points from $\phi = \pi$ to $\phi = 2\pi$ are either removed from the influence of the current (when circular arcs are obtained for such portions of the margin), or if subject to the influence of the current (by a slight obliquity in the plane of the leaf, the anterior margin being depressed), would have it so as to assist rather than oppose the power of growth, and the equation for such portion of the margin would be

$$\frac{ds}{d\phi} = \frac{\tau}{p + \frac{1}{2} s v^2 \sin^2\phi};$$

assume
$$\tan^2\gamma = \frac{\sigma v^2}{2p},$$

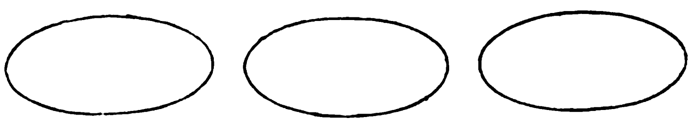
then
$$\frac{p}{\tau} \,.\, s = \int \frac{d\phi}{1 + \tan^2\gamma \,.\, \sin^2\phi} = \int \frac{d \,.\, \tan\phi}{1 + \sec^2\gamma \,.\, \tan^2\phi}$$

$$= \cos\gamma \,.\, \tan^{-1} .\, (\sec\gamma \,.\, \tan\phi),$$

$$\frac{p}{\tau} \,.\, x = \int \frac{d \,.\, \sin\phi}{1 + \tan^2\gamma \,.\, \sin^2\phi} = \frac{1}{\tan\gamma} \,.\, \tan^{-1} .\, (\tan\gamma \,.\, \sin\phi),$$

$$\frac{p}{\tau} \,.\, y = -\int \frac{d \,.\, \cos\phi}{\sec^2\gamma - \tan^2\gamma \,.\, \cos^2\phi} = \frac{\cos^2\gamma}{2\sin\gamma} \,.\, \log .\, \frac{1 - \sin\gamma \,.\, \cos\phi}{1 + \sin\gamma \,.\, \cos\phi};$$

therefore
$$e^{\frac{\sin\gamma}{\cos^2\gamma} \cdot \frac{p}{\tau} \cdot y} + e^{-\frac{\sin\gamma}{\cos^2\gamma} \cdot \frac{p}{\tau} \cdot y} = \frac{\pm 2}{\cos\gamma} \,.\, \cos .\left(\tan\gamma \,.\, \frac{p}{\tau} \,.\, x\right).$$



Suppose that the leaf-margin after acquiring the shape as determined above ceases to be flexible, then the action between two contiguous portions of the margin will not be entirely tangential, but in addition to the tension τ there will exist a normal strain N and a mechanical couple P tending to wrench the margin. Let τ_0, p_0, v_0 be the tension, the power of growth, and the velocity respectively at the time when the margin acquires its shape, then for portions of the margin subject to the opposing resistance of the current,

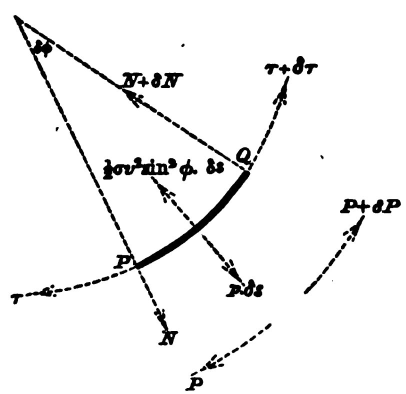
$$\frac{ds}{d\phi} = \frac{\tau_0}{p_0 - \frac{1}{2}\sigma v_0^2 \sin^2\phi}.$$

The new equations of equilibrium will be

$$-\tau + (\tau + \delta\tau)\cos\delta\phi - (N + \delta N)\sin\delta\phi = 0,$$

$$N - (N + \delta N)\cos\delta\phi - (\tau + \delta\tau)\sin\delta\phi$$

$$+ \left(p - \frac{1}{2}\sigma v^2 \sin^2\phi\right)\delta s \, . \cos\frac{\delta\phi}{2} = 0,$$



and by taking moments about the point P

$$P - (P + \delta P) - (N + \delta N)\,\delta s = 0,$$

and passing to the limit

$$\frac{d\tau}{d\phi}=N, \quad \frac{dN}{d\phi}+\tau=\left(p-\frac{1}{2}\sigma v^2.\sin^2\phi\right)\frac{ds}{d\phi}, \quad \frac{dP}{ds}+N=0.$$

Therefore $$\frac{d^2\tau}{d\phi^2}+\tau=\tau_0.\frac{p-\frac{1}{2}\sigma v^2.\sin^2\phi}{p_0-\frac{1}{2}\sigma v_0^2.\sin^2\phi}$$

$$=\tau_0.\frac{v^2}{v_0^2}+\frac{\tau_0}{v_0^2}.\frac{v_0^2 p-p_0.v^2}{p_0-\frac{1}{2}\sigma v_0^2.\sin^2\phi}.$$

Therefore (see Boole, *Differential Equations*, edit. I. p. 383)

$$\tau=\frac{v^2}{v_0^2}.\tau_0+\frac{\tau_0.\sin\phi}{v_0^2}.\int\frac{\cos\phi\,(v_0^2p-v^2p_0)\,d\phi}{p_0-\frac{1}{2}\sigma v_0^2\sin^2\phi}-\frac{\tau_0\cos\phi}{v_0^2}\int\frac{\sin\phi(v_0^2p-v^2p_0)\,d\phi}{p_0-\frac{1}{2}\sigma v_0^2\sin^2\phi}$$

$$=\frac{v^2}{v_0^2}.\tau_0+\sin\phi\int\frac{dx}{ds}.\frac{(v_0^2p-v^2p_0)}{v_0^2}.\frac{ds}{d\phi}.d\phi-\cos\phi\int\frac{dy}{ds}\frac{(pv_0^2-p_0v^2)}{v_0^2}.\frac{ds}{d\phi}.d\phi.$$

Therefore if p may be considered constant for different points

$$\tau=\frac{v^2}{v_0^2}.\tau_0+\frac{(v_0^2p-v^2p_0)}{v_0^2}\sin\phi\,(x-a)-\frac{(pv_0^2-p_0v^2)}{v_0^2}\cos\phi\,(y-b),$$

where a and b are constants;

$$N=\frac{d\tau}{d\phi}=\frac{(v_0^2p-v^2p_0)}{v_0^2}\sin\phi.\cos\phi.\frac{ds}{d\phi}+\frac{(v_0^2p-v^2p_0)}{v_0^2}\cos\phi\,(x-a)$$

$$-\frac{(pv_0^2-p_0v^2)}{v_0^2}\cos\phi.\sin\phi\,\frac{ds}{d\phi}+\frac{(v_0^2p-v^2p_0)}{v_0^2}\sin\phi\,(y-b)$$

$$=\frac{(v_0^2-v^2p_0)}{v_0^2}.\{\cos\phi.(x-a)+\sin\phi.(y-b)\};$$

$$P=-\int N.ds=-\frac{(v_0^2p-v^2p_0)}{v_0^2}\int\left\{\frac{dx}{ds}(x-a)+\frac{dy}{ds}(y-b)\right\}.ds$$

$$=\frac{v_0^2p-v^2p_0}{2v_0^2}\{c-(x-a)^2-(y-b)^2\},$$

where c is a constant.

But since from the nature of the case both τ and P are unaffected when $\pi - \phi$ is written for ϕ, therefore $b = 0$. Thus τ, N, and P are determined; and it is easily seen that the variable portion in the expression for the tendency to break varies as the square of the distance from a point situated in the axis of x.

The theory advanced in this paper applies to leaves whose structure is such that the marginal vigour of growth is the same at all points; and in the case of aquatic plants with floating leaves which may be otherwise organized, some modification of the same theory will be necessary; it applies during the period between the first unrolling of the leaf at the surface of the water and the completion of its growth. Each lobe of a divided leaf must be treated to a separate calculation, and new lobes may be formed at those points where the tendency to break is the greatest. Growth is supposed to proceed steadily, while the leaf is submitted to a suitable external pressure; when violent pressures exist, as, for example, when the current is very rapid after heavy rains or otherwise, growth is probably checked for a time and fissures may be started, or the leaf-margin maintains its shape by the support of its interior which then resists the external pressure, until at a proper time growth pushes forward the margin again, and its form is matured in obedience to the above-investigated laws.

It is further to be noted that the form of the leaf-margin remains the same, even if at different times the power of growth, the direct resistance due to the velocity of the current, and the marginal tension all vary, provided only that their proportional values remain unaltered. It is a tenable hypothesis and by no means improbable that, during much or most of the time when actual growth is taking place and when the velocity of the current is subject to many and various vicissitudes, the plant has the power of adapting its growing efforts to the circumstances just necessary for its development, that is, in the no-

tation of the previous analytical investigation. the quantities p, σv^2, τ, or at all events the first two of them, maintain a constant proportion. The shape of the curve depends only upon the ratio of p to σv^2, and the size depends further upon the proportional value of τ.

It is evident, on the other hand, that neither any one curve nor the system of curves belonging to any one of the above equations nor any portion of it or them can, except in very simple and entire leaves, delineate the whole margin of the floating leaf; for otherwise there would be no means of explaining the divisions, fissures and incisions which are frequent even in floating leaves and which give characters for the definition of species. It does not therefore follow as a consequence of this investigation that all floating leaves grown in a perfectly still water (if such a phenomenon were possible to contrive) are simply circular in outline, though a circular form might be favoured; but it does follow that the several portions of the margin would be circular arcs.

It has before been hinted at, that new fissures (in addition to any previously existing ones) may be made and accounted for by the mechanical action of the current.

It is a matter of common observation that many floating leaves, as for example in *Ranunculus*, vary considerably in consequence of, or in association with, the nature of the stream in which they grow.

At all events the theory discusses the forms which floating leaves would find mechanically suitable for their growth and maintenance, in order that they might dwell free from unnecessary strains and wrenches, and under an equal distribution of their power of growth, which as we know is capable of exerting considerable force under compulsion, but is in general slow and steady.

ANNUAL GENERAL MEETING, OCTOBER 30, 1871.

The PRESIDENT (PROFESSOR CAYLEY) in the Chair.

The following Officers were elected:

President.

Professor HUMPHRY.

Vice-Presidents.

Professor C. C. BABINGTON.
Professor CAYLEY.
Professor ADAMS.

Treasurer.

Dr CAMPION.

Secretaries.

Mr BONNEY.
Mr J. W. CLARK.
Mr COUTTS TROTTER.

New Members of the Council.

Professor MILLER.
Professor MAXWELL.
Mr GODFRAY.
Mr J. STUART.

Communications made to the Society:

(1) *On the Equation which determines the form of the Strata in Legendre's and Laplace's Theory of the Figure of the Earth. By* I. TODHUNTER, M.A., F.R.S.

The object of this Memoir is to examine various investigations which have been given respecting the equation which occurs in the theory of the figure of the Earth considered as a heterogeneous fluid, and from which it is inferred that the figure must be that of an ellipsoid of revolution. Especially the assumptions on which these investigations rest are discussed. The most general treatment which the equation has hitherto received is shewn to be unsound. Finally a new method is proposed, by which the required result is demonstrated with fewer limitations than have hitherto been employed.

(2) *On a Cirque in the Syenite Hills in the Isle of Skye. By* T. G. BONNEY, B.D.

The Syenite hills occupy a portion of the eastern coast of Skye between the Liassic plain of the Strath (through which they have been extruded) and the great Trap district on the north. Though the date of this extrusion is uncertain, it is generally believed to have happened—as did that of the Trap—in Meiocene times. The author stated that he had already described a number of cirques in districts of sedimentary rock (*Quarterly Journal of the Geological Society*, Vol. XXVII. p. 312); he was now able to bring forward an instance from the crystalline rocks, in which good examples of such configurations, so far as his experience went, were rare. He considered that the cirques described near the heads of Alpine valleys could not be accounted for by upheaval, or marine erosion, and, by reason of the steepness of their cliffs and the limited space above

them, could not have been excavated by glaciers. In the abovenamed paper he had brought forward reasons for maintaining that cirques were excavated by numerous rather small streams, acting on rocks, suitably stratified, whose composition and arrangement admitted of considerable meteoric erosion. This cirque in Skye, a double one, viz. Є-shaped, had its cliffs seamed by the tracks of numerous streamlets, each with its little talus of débris resting on sloping glacier-worn rocks below. He held, therefore, that this cirque had been brought to its present state by the action of streamlets, fed by rains; and had to a large extent been pre-glacial, seeing that the floor was ice-worn. Its configuration forbade him to attribute it to a glacier, unless this agent could be invested with a power of eroding vertically.

Mr O. FISHER said the author had shewn the glacier erosion theory would not hold, but he thought that vertical cliffs must necessarily be formed by being attacked from the bottom, and that streams pouring down from above would have a tendency to produce a talus and so to mask rather than to form a cliff. He called attention to the action of the sea as evidenced in the Alps.

Professor MILLER mentioned an instance shewing how slight the excavating power of water often was: at Bamberg, veins of quartz in a rock scarped some 800 years ago, and since then weathered, now protrude only from $\frac{1}{8}$ to $\frac{1}{2}$ an inch.

Professor LIVEING thought that streams could only cut away the bottom of a talus, when they were shot out by an overlying sheet of ice.

Mr BONNEY, in reply, thought it possible that in many cases there had been a pre-existing favourable configuration of the ground, but how that was produced there was now nothing left to tell. He said that streams in flood could move débris from slopes or taluses where they had deposited it when at their usual volume; that it was impossible to lay down a general rule as to either the rate of erosion at any place—(he had no faith

in the application of the Rule of Three to geological chronology),—or whether the transporting force of a stream was greater than, less than, or equal to its denuding force: each case had to be judged by itself. In many cases he thought that at the present day the taluses were increasing on the cirques; that, however, was only a question of rainfall, strata, and the like: further it must not be forgotten that there is chemical as well as mechanical denudation.

The MASTER of EMMANUEL asked whether there was an observed difference of constitution in the rocks to explain either the floor or a difference in the slope in the walls of the cirque.

Mr BONNEY replied that it was in the case of most cirques difficult to say positively, seeing that their floors were often masked by talus, vegetation, &c., and that in the case of the one described in the paper, in the Syenite, there was no distinction that was conspicuous, although there very probably was some in the chemical constitution of the rock. Differences in the slope of the walls of a cirque in sedimentary rocks were doubtless due to difference in the strata.

Some further conversation took place in which Mr Potter, Mr O. Fisher, and others took part.

November 13, 1871.

The PRESIDENT (PROFESSOR HUMPHRY) in the Chair.

Communications were made to the Society:

(1) *On a double action Pentagraph. By* Prof. CAYLEY, F.R.S.

The machine was exhibited and described.

(2) *On the experimental verification of the laws of the resistances which bodies are subject to, in moving through the air; and especially on the experiments made by Mr Robins and Dr Hutton with the* Whirling Machine. *By* R. POTTER, M.A.

The experiments made with the Whirling Machine give the resistances which bodies whirled round in the air experience to considerable nicety, and have been considered as correctly giving the resistance which the same bodies respectively would have experienced if they had moved in straight lines. In this paper it was shewn that the centrifugal force communicated to the air, as is continually seen in various winnowing machines and blowing machines, required consideration, and that the experiments must be separated from those in which bodies move nearly in a straight line through the air.

(3) *On the comparison of the results given by the formula for the resistances which bodies experience whilst moving through fluids, investigated in the paper read before the Society on 7th March, 1870, with the experimental results found by Mr Robins and Dr Hutton by means of the Ballistic Pendulum. By* R. POTTER, M.A.

In this paper computations from the mathematical formulæ obtained in a paper read before the Society on 7th March, 1870, were compared with the results found experimentally by Mr Robins and Dr Hutton with the Ballistic Pendulum, to determine the resistance which musket and cannon shot experienced on passing through the air. It was shewn that the computed and experimental results agreed as closely as could be expected in such a subject, and that we may now maintain that the refractory problem of resistances to motion in the air has at length been solved mathematically.

Mr GLAISHER mentioned some observations in which he had been engaged, which shewed that the formula for resistance $p = kv^2$ did not hold in several cases, especially where p was

small, and said that the Aeronautical Society was engaged in repeating the experiments with the Whirling Machine.

Prof. CLERK MAXWELL mentioned that when the Ballistic Pendulum was used, there was a difficulty in accurately obtaining the initial velocity of the shot fired, but in other experiments, viz. those of Mr Bashforth, the shot was fired through frames with cotton threads; these were far better, as then variation in the initial velocity of the shot was avoided.

Some further conversation took place in which Mr Stuart, Prof. Clerk Maxwell, and Mr Potter took part.

Nov. 27, 1871.

The PRESIDENT (PROFESSOR HUMPHRY) in the Chair.

New Fellows elected:

B. E. HAMMOND, M.A. *Trinity College.*
W. A. BRAILEY, M.A. *Downing College.*

Communications made to the Society:

(1) *On the Solution of Electrical Problems by the Transformation of Conjugate Functions. By* Prof. CLERK MAXWELL, F.R.S.

The general problem in electricity is to determine a function which shall have given values at the various surfaces which bound a region of space, and which shall satisfy Laplace's partial differential equation at every point within this region.

The solution of this problem, when the conditions are arbitrarily given, is beyond the power of any known method, but it is easy to find any number of functions which satisfy Laplace's equation, and from any one of these we may find the form of a system of conductors for which the function is a solution of the problem.

The only known method for transforming one electrical problem into another is that of Electric Inversion, invented by Sir William Thompson; but in problems involving only two dimensions, any problem of which we know the solution may be made to furnish an inexhaustible supply of problems which we can solve.

The condition that two functions α and β of x and y may be conjugate is

$$\alpha + \sqrt{-1}\,\beta = F(x + \sqrt{-1}\,y).$$

This condition may be expressed in the form of the two equations

$$\frac{d\alpha}{dx} - \frac{d\beta}{dy} = 0, \qquad \frac{d\alpha}{dy} + \frac{d\beta}{dx} = 0.$$

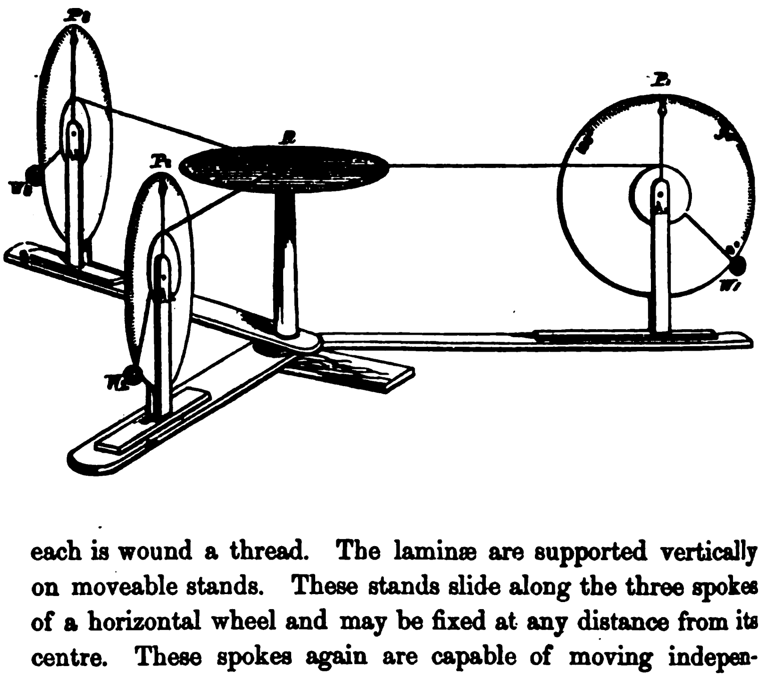
If α denotes the "potential function," β is the "function of induction." As examples of the method, the theory of Thomson's Guard Ring and that of a wire grating, used as an electric screen, were illustrated by drawings of the lines of force and equipotential surfaces.

Professor CAYLEY pointed out a theory of the translation of figures, the small parts of which are the same, which Prof. Maxwell in his paper appeared to be leading up to.

Prof. MAXWELL replied that he had prepared a diagram with the purpose of illustrating this case of the transformation of Conjugate Functions.

(2) *On a machine for illustrating the "Parallelogram of forces."* *By* J. C. W. Ellis, M.A.

It consists of three graduated circular laminæ. To the rim of each at the point 0° is fixed a weight *w*. Round the axle of



each is wound a thread. The laminæ are supported vertically on moveable stands. These stands slide along the three spokes of a horizontal wheel and may be fixed at any distance from its centre. These spokes again are capable of moving independently around their common vertical axis so as to take up any angular position with regard to each other. *P* is an index fixed vertically above the centre of each lamina. It can be easily proved experimentally that the tension of the thread will be proportional to the sine of the angle between *W* and *P*. The three threads are all joined to a little ring *R* so as to rest horizontally on a horizontal circular graduated lamina attached

to the end of the vertical axis of the wheel whose moveable spokes support the laminæ.

Let T_1, A_1, P_1, W_1 be respectively the tension of thread, the centre, the pointer and the weight fixed to the rim of one lamina, and so of the others.

To use the machine—Place a pin through the ring R so as to fix the ring to the centre of the graduated horizontal lamina. Revolve the spokes so that the angles A_1RA_2, A_2RA_3, A_3RA_1 as read off on the horizontal lamina may be any selected. Slide out the vertical laminæ until by the tension of the threads the weights rise so that

$$\angle P_1A_1W_1 = \angle A_2RA_3,$$
$$\angle P_2A_2W_2 = \angle A_3RA_1,$$
$$\angle P_3A_3W_3 = \angle A_1RA_2.$$

But
$$\frac{T_1}{\sin P_1A_1W_1} = \frac{T_2}{\sin P_2A_2W_2} = \frac{T_3}{\sin P_3A_3W_3};$$

$$\therefore \frac{T_1}{\sin A_2RA_3} = \frac{T_2}{\sin A_3RA_1} = \frac{T_3}{\sin A_1RA_2},$$

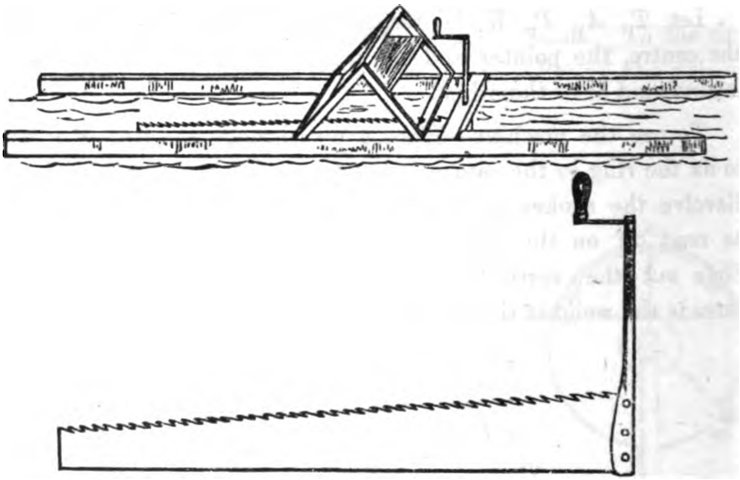
or the tensions of the threads are as the sines of the angles between the other two. Now remove the pin, and the system will be found to be in equilibrium.

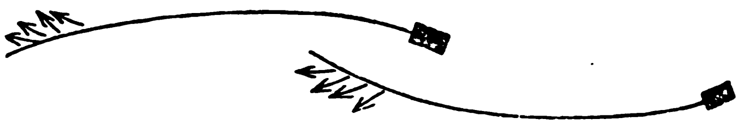
(3) *On a mode of propelling vessels. By* J. C. W. Ellis, M.A.

In a fish the tail is the great propelling machine, the office of the fins is merely to guide and balance. It is rather difficult to conceive this action of the tail which is able to drive the body through the resisting medium at such great speed.

The single oar at the stern of a boat as used by seamen, or one at the side as used at Venice or in the Upper Rhine, produces motion in a way not altogether unlike that of the tail of a fish.

The screw of a steamer acts also in a similar way, and perhaps this is the most perfect machine for the purpose of





propulsion that can be invented. But as rapid rotatory motion is requisite to drive the screw with effect, the machinery is not of the simplest kind.

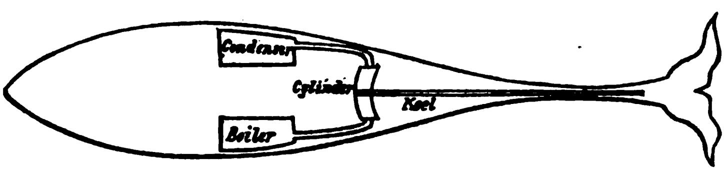
A method of propelling has been tried, where motion is obtained by driving water through a tube astern. This, however, is but another form of the screw.

Another method has been proposed where the action of the foot of a water-fowl is imitated.

The present method is one founded on an attempt at a closer imitation of the action of a fish's tail.

The following is the result of some experiments made about two years ago on a lake in Wales. Two wooden pipes about 26 feet long were covered with canvas and tarred: they were fixed parallel about 1 yard apart, and a seat raised above them.

This formed the boat. A crank-handle was attached to a vertical shaft opposite the seat. The lower end of the shaft was about 7 inches below the surface of the water. This end was so contrived that boards of various shapes and sizes to imitate a fish's tail might be attached at pleasure so as to lie horizontally just under the surface of the water. A number of deal boards of various lengths, taper and shape were tried successively, but with not much success. Finally a large steel saw about 8 feet long was introduced. This was so far successful that had the saw been broader, and stiffer at the fixed end, it was clear that fair results might be anticipated. As it was, with very slight exertion, although the boat was very heavy, being nearly water-logged by water having found its way into the pipes, the boat moved forward at the rate of about two miles per hour. The action of the saw seemed to be precisely that of the tail of a fish, lashing from side to side and driving the water astern. When the crank-handle was turned to one side the saw was bent, and in trying to recover itself produced a pressure upon the water partly to one side and partly astern: it was the reaction of this latter portion which drove the boat forward. Any other form of boat would have done equally well, the upright shaft taking the place of the rudder. A pliant steel rod covered with India rubber or gutta-percha in the form of a tail might be even more successful. Within the last few months it has been reported in the newspapers that boats propelled in this way have been tried successfully on some of the American canals.



If such a mode of propulsion could be introduced with success, the machinery for driving it might be very simple

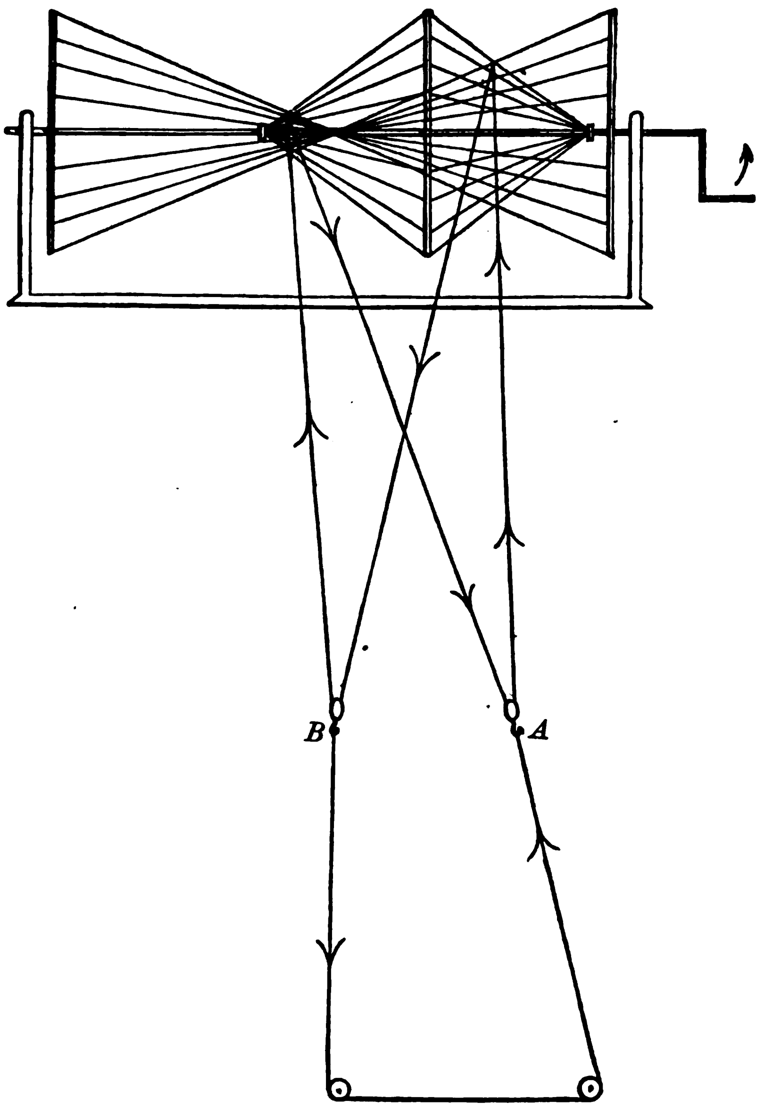
indeed. We should not require rotatory motion. The tail might be attached directly to the middle of a cylinder sliding backwards and forwards on a fixed piston-rod, the cylinder and piston-rod forming small arcs of circles, the piston-rod being also the steam-pipe.

(4) *On a method of transforming rotutory motion into rectilinear, so that the rotatory motion remaining constant the rectilinear may be completely controlled and made to vary as to speed—may be stopped or reversed at pleasure.* *By* J. C. W. Ellis, M.A.

Two equal cones with their vertices fixed together forming a double cone have a common axis. On this same axis there are two other cones with their bases fixed together so that they form another double cone. All these cones are formed of bars, so that the vertex of the first double cone can lie in the interior of the second double cone. The axis is not attached to the first double cone, but is to the second, so that sliding the axis in direction of its length the position of the cones may be altered with regard to each other. The intersections of the cones form two wheels, the sum of whose radii is constant, but the radii may have any ratio to each other.

An endless rope is passed *over* one of these wheels, round a distant moveable pulley A, *under* the other wheel, half round it, then round another moveable pulley B, and then *under* the first wheel. The two moveable pulleys may be connected by a rope passing over fixed pulleys.

If the cones be now made to revolve and the double cones be placed symmetrically, the wheels they form will have equal radii and the pulleys A and B will remain in their positions as the endless rope runs round. But if the common axis of the cones be made to slide, the radii of the wheels will alter and the pulley A can be made to approach or recede from the cones with any rapidity according to pleasure, without disturbing the


B
A

uniform rotation of the cones. A cam might be made to act upon the common axis so as to slide it in or out according to any given law, and so produce any required motion in the pulley *A*. There are endless varieties of uses to which this device might be applied. It was originally designed in order to reduce the rapid rotatory motion of a circular saw into a slow rectilinear motion in order to bring the timber up to the saw, in such a way that the advance might be instantly stopped or reversed, or might be made slower to suit the nature of the timber employed. Some such contrivance might possibly be useful for ploughing purposes, so as to enable lighter tackle to be used than at present, and to regulate the speed of the plough according to the nature of the ground.

Prof. CAYLEY asked whether the third machine could be made accurate enough to allow it to be applied to the work of a pentagraph, for theoretically it was capable of it.

MONDAY, *Feb.* 12, 1872.

THE PRESIDENT (PROFESSOR HUMPHRY) in the Chair.

It was announced by the President that the Adjudicators of the Hopkins Prize (Professors Stokes, Tait, and Clifton) had awarded it to Professor J. Clerk Maxwell.

It was also announced that Vol. XI. Part 3 of the Society's *Transactions* was now ready, and would be delivered to the members on application.

Communications were made to the Society:

(1) *Further observations on the state of an eye affected with a peculiar malformation. By* THE ASTRONOMER ROYAL.

In this paper the author gave numerical results derived from measurement of the astigmatism of an eye, extending over a considerable period of years, which shewed that during this time there had been a change in the astigmatism.

Mr PAINE observed that he had found from his experience as an optician that about 1 in 100 suffered perceptibly from astigmatism, and described the mode of correcting it by glasses. He said that when astigmatism existed in the crystalline lens it was difficult to remedy it; but not so when it was in the cornea. It was generally supposed that the astigmatism did not alter with age, so that the Astronomer Royal's observations were of much interest, as they shewed a change.

Professors Miller and Maxwell, Mr Trotter and Dr Latham also made some brief remarks on the subject of the paper.

(2) *The comparison of measures* à traits *with measures* à bouts. *By* Prof. MILLER, F.R.S.

A standard of length in which the measure is defined by the distance between certain points in the surfaces by which the two ends of a material bar are respectively bounded is called a measure *à bouts*. A standard in which the measure is defined by the distance between two fine lines traced at right angles to the axis of the bar is called a measure *à traits*. The methods of comparing with one another two measures of the same kind are well known and need not be alluded to here. But the comparison of a measure *à traits* with a measure *à bouts* cannot be effected so readily. The Astronomer Royal, on being consulted respecting the best method of making such a comparison, recommended the following process. Two copies, *AB, DE,* of the original standard *à bouts* are constructed with cylindrical cavities at their middle points reaching down to the points *C, F* in the axes of the bars, where lines are traced at right

angles to their axes, like the cavities adopted by Mr Baily, at the writer's suggestion, near the ends of the standard yards and their copies. Let s denote the length of the original standard, c that of the copy *à traits*.

Then, the end D of the bar DE being brought into contact with the end B of AB, the distance CF may be compared by microscopes with c. Again, A being brought into contact with E, FC can be compared in the same manner with c. And AB, DE may be compared with s by touch. From the data thus obtained the difference between s and c may be readily found.

The cavities formed in the middle points of the bars must weaken them so as to materially injure their value except for the single operation described.

The same end may however be attained without the necessity of sinking cavities down to the axes of the bars. The object of the Observer is to obtain a visible mark invariably fixed for some hours or some days in the axis of each bar near its middle point.

At the middle of each bar let a right-angled prism be attached with its two smaller faces parallel to the vertical and horizontal faces of the bar supposed to be square. Attach also a small plate of glass, having a fine line traced upon it, in such a position that the line seen by total reflexion in the prism may appear to cross the middle point of the axis of the bar at right angles to it.

The marks thus obtained in the axes of the bars may now be used instead of the lines C, F traced in cavities sunk in the bars, and the operation of comparing c with s will be exactly the same as that which has been already described.

In a Memoir by Steinheil on the construction of a *comparateur* for measures *à bouts* in the 27th Volume of the *Denkschriften der Akademie der Wissenschaften* of Vienna, page 166, it is stated that the probable error of a comparison by touch is 0·00005 millimètres, and that of a single comparison

by microscopes is not less than 0·0005 millimètres. This accuracy is however illusory at temperature 0° *C*, unless the mercurial thermometer used in finding the expansion of the bar, or an accurate copy of it, is preserved for subsequent use. For Regnault's observations shew that thermometers constructed of different kinds of glass, though in perfect accordance with one another near 0° *C* and 100° *C*, may differ 0°·5 *C* at 50° *C*, as pointed out by J. Bosscha, jun. (*Archives Néerlandaises*, T. IV.; Poggendorff's *Annalen, Ergänzungsband*, V., 1871, page 465). At $16\frac{2}{3}$° *C*, the standard temperature adopted in this country, the difference may amount to 0°·28 *C*, which implies an uncertainty of 0·0045 millimètres in the length of a bronze yard, or nine times the probable error of a single microscopic comparison of two such bars, and ninety times the probable error of a single comparison of two end yard bars by touch.

The necessity for appealing to the thermometer used in the original comparison of a bar, in order to find the length of the bar at any given temperature, may be obviated by observing the expansion of the bar from 0° *C* to 100° *C*, and from 0° *C* to about 50° *C*, by the original thermometer. The same observations being afterwards made with a second thermometer, even if of different glass, and differing in its readings from the original thermometer at points intermediate between 0° *C* and 100° *C*, we have data from which we can deduce the temperature, as indicated by the original thermometer, from the reading of the new one, and thus obtain the true length of the bar at a temperature considerably distant from 0° *C*.

Professor MAXWELL asked what the value of Whitworth's method was.

Professor MILLER replied that the instrument was so delicate that it was only of use when most carefully handled.

Some further conversation occurred in which Mr Ellis, Prof. Maxwell, and Prof. Miller took part.

MONDAY, *Feb.* 26, 1872.

The PRESIDENT (PROFESSOR HUMPHRY) in the Chair.

Fellows elected:

J. W. HICKS, B.A., *Sidney Sussex College.*
E. H. MORGAN, M.A., *Jesus College.*

Communications were made to the Society:

(1) *On Teichopsia, a form of transient 'halfblindness;' its relation to nervous or sick headaches, with an explanation of the phenomena. By* P. W. LATHAM, M.D.

The author said that the disturbance of vision referred to in this paper was a subject which had engaged the attention of Sir John Herschel, the Astronomer Royal, Dr Hubert Airy, and many members of the medical profession. He should proceed to shew that it was one stage of a complaint known under the name of nervous headache, bilious headache or sick headache; the complaint not always accompanied by disturbed vision, but other disordered sensations being substituted for it, and on the other hand the disturbed vision not being always followed by headache; and he should then endeavour to explain the phenomena. He divided the complaint into two stages, (i) the stage of disordered sensation, and (ii) the stage of headache. After quoting the descriptions given by those whose names are mentioned above as well as by persons who had come under his own observation, he referred to the causes and conditions under which the attacks were induced. It is to be observed, he said, that all these causes and causes like to them are of a depressing nature, exhausting the power, and therefore lowering the tone of the system, putting it out of tune, disturbing the harmony of the functions, and at the same time exalting

the susceptibility of the nervous system. The result was that the power of the ganglia of the sympathetic nervous system to conduct, transfer and radiate the effects of impressions, was no longer controlled by the superior force in the cerebro-spinal centres, and instead of tranquil even harmonious action in the various organs as in perfect health, we had convulsive and painful movements. After referring to the effects of irritation and section of branches of the sympathetic, the next step in his argument was that in the disorder under consideration there was first of all *contraction* of the vessels of the brain (probably the middle cerebral artery), and so a diminished supply of blood produced by excited action of the sympathetic, and that the exhaustion of the sympathetic following on this excitement causes the *dilatation* of the vessels and the headache. This he supported by various cases and comparisons. He next discussed the question, why the disorder might be sometimes unilateral and sometimes bilateral, and lastly, why in some cases there is (i) disturbance of vision without headache following, (ii) disturbance of vision followed by headache, and (iii) headache preceded by disordered sensation, but not by disturbed vision; all of which he maintained were explicable by the theory which he had advanced.

Prof. HUMPHRY said that he had experienced a sudden attack of hemiopia, which was probably of somewhat similar origin; that on another occasion he had been conscious of considerable mental disturbance, accompanied by dilatation of a pupil. This he believed due to the eye being accidentally touched by a little atrophine, and that the mental disturbance was merely nervous sympathy.

Mr TROTTER asked whether Dr Latham could suggest any cause for the peculiar complicated figure appearing in this disorder.

Dr LATHAM said he could not explain the special form.

Prof. MILLER stated that he himself had sometimes seen the

zizgag outline described, very faint and shadowy, without any other disturbance of the system; it lasted about ten minutes.

Dr MICHAEL FOSTER asked whether Dr Latham had had the opportunity of seeing any patient during the attack itself, so as to see how far the blood-vessels and the pupils were affected.

Dr LATHAM said that he had recently seen two cases during the headache, the pupil was then contracted; but he had not lately seen a case during the time of the peculiar affection of the vision.

Dr M. FOSTER said that though Dr Latham had given a general explanation, he had not brought the explanation sufficiently near to the particular case; that the special description of teichopsia could only be explained when we knew something more about a very complex matter, the vaso-motor centres of the brain; he also thought that the phenomena described might possibly be produced by some affection of the circulation in the retina.

Dr LATHAM said he was quite aware that some difficulties yet remained in his explanation—he had indeed thought that the minor cases of the disorder might be produced by some affection of the ophthalmic artery.

The PRESIDENT said that he was not inclined to attribute it to the ophthalmic artery, but rather to the brain, whether vascular or not could not yet be said.

(2) *A machine for tracing Curves described by points of a vibrating String; namely, curves of the forms*

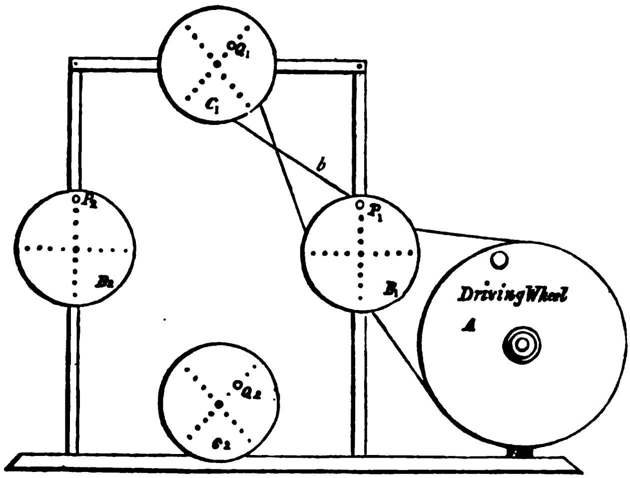
$$x = a \cos\left(\frac{2\pi}{\tau_1} t + \alpha\right), \qquad y = b \sin\left(\frac{2\pi}{\tau_2} t + \beta\right),$$

and curves of the form

$$x = a\cos\left(\frac{2\pi}{\tau_1}t+\alpha\right) - b\cos\left(\frac{2\pi}{\tau_2}t+\beta\right),$$

$$y = a\sin\left(\frac{2\pi}{\tau_1}t+\alpha\right) - b\sin\left(\frac{2\pi}{\tau_2}t+\beta\right),$$

when α *and* β *are constants, or* $\alpha \sim \beta$ *a fraction of* t + *a constant.* By J. C. W. ELLIS, M.A.



A driving wheel (A) drives by means of a band a disk B_1. B_2, C_1, C_2 are three other disks equal to B_1. A, B_1, B_2, C_1, C_2 are all in a vertical plane. The centres of B_1, B_2 are fixed in the same horizontal line at a distance of about four times the diameter of the disks. The centres of C_1, C_2 are similarly placed in a vertical line. B_1, B_2 are so connected by rods or otherwise so as to revolve simultaneously. So are C_1, C_2. The axis of B_1 is also the axis of a cone with the vertex pointing from B_1 and revolving with B_1. This cone drives by means of a band (b) the disk C_1, which is attached to a similar cone with its vertex pointing towards C_1. In the disks are bored a number of equidistant holes along the diameters, into which

can be inserted pegs P_1, P_2 and Q_1, Q_2. P_1, P_2 are fixed at equal distances from the centres in B_1, B_2: Q_1, Q_2 in C_1, C_2.

Now as B_1, B_2 revolve through equal angles in equal times, the line P_1P_2 will always move parallel to itself, and similarly Q_1Q_2.

Hence, if O be the intersection of the lines joining the centres, and P of P_1P_2 and Q_1Q_2; and straight lines in the plane of the disks through O be taken as axes of co-ordinates, the equation to the locus of P will be

$$x = a\cos(m\theta + \alpha_1),$$
$$y = b\sin(n\theta + \beta_1),$$

where b, which can be varied at pleasure, is the distance of the peg P_1 from the centre of B_1, and a of Q_1 from the centre of C_1.

$m : n$ are the velocity ratios of rotation of the disks C_1 and B_1. This ratio may be altered at pleasure by shifting the band along the cones. Either m or n is negative if the bands are crossed. The above equation to the locus of P may be written (as in Donkin's *Acoustics*):

$$x = a\sin\left(\frac{2\pi}{\tau_1}t + \alpha\right), \qquad y = b\sin\left(\frac{2\pi}{\tau_2}t + \beta\right),$$

where t is the time, τ_1 the time of revolution of the disk C_1, τ_2 of B_1, $\alpha = \frac{\pi}{2} + \alpha_1$, $\beta = \beta_1$.

If τ_1 and τ_2 are commensurable then the locus of P is a re-entering curve; if not, not.

If τ_1, τ_2 are nearly in the ratio of two small numbers m and n, the curve though not re-entering after the time mn may be expressed by the equation

$$x = a\sin\left(\frac{2\pi}{m}t + \alpha + kt\right), \qquad y = b\sin\left(\frac{2\pi}{n}t + \beta\right),$$

where k is a small quantity, so that $\alpha + kt$ represents a slow

change of α during the course of each revolution. The curve described, instead of re-entering exactly at the time mn, does so nearly but not accurately, owing to the small change in α, so that it starts on a slightly different course after each interval mn. So that the appearance is that of a curve, slowly changing its character and position, until, if τ_1 and τ_2 are commensurable, it finally returns to its primitive state. If τ_1 and τ_2 are not commensurable it never does so.

All these points can be clearly indicated by fixing a screen so as to cover the disks: having in it two slits at right angles to each other, one in which P_1 and P_2 slide, and the other in which Q_1, Q_2 slide. It is manifest that any point in this screen will trace out the curve

$$x = a \sin\left(\frac{2\pi}{\tau_1} t + \alpha\right), \qquad y = b \sin\left(\frac{2\pi}{\tau_2} t + \beta\right),$$

when a and b can be arranged at pleasure by shifting the pegs, τ_1, τ_2 by shifting the band, and α, β by arranging the position of the disks at starting. A pencil pressed against the screen will trace out the curves.

The form of the curves may also be represented to the eye by piercing a hole in the screen, and placing a strong light behind it; or by means of the electric spark passing between the ends of fine wires, the ends being fixed close together at any point on the screen.

Another way of representing the curve to the eye is this: two screens are provided, one is attached to the disks B_1, B_2 by the pegs P_1, P_2; P_1, P_2 do *not slide* in a slit, but there is a slit extending nearly from P_1 to P_2. Similarly in the screen attached to C_1, C_2 there is a slit extending nearly between Q_1 and Q_2. Light if placed behind can be seen through both screens at once only at the point where the slits cross, namely, at the point P, whose locus is the point we have been treating of.

If to one of these screens a pencil be attached it will trace out on the other screen the curve

$$x = a\cos\left(\frac{2\pi}{\tau_1}t + \alpha\right) - b\cos\left(\frac{2\pi}{\tau_2}t + \beta\right),$$

$$y = a\sin\left(\frac{2\pi}{\tau_1}t + \alpha\right) - b\sin\left(\frac{2\pi}{\tau_2}t + \beta\right).$$

The constants in this can be arranged at pleasure as in the former curve.

It is manifest that if $a = b$ and $\tau_1 = \tau_2$ the curve is reduced to a point, and the corresponding vibration to rest, *i. e.* the composition of the two motions of the paper and pencil produce rest.

This may be taken as an example of interference.

The pencil and every point in the paper are describing the same circle, so that there is no relative motion, and the pencil does not travel over the paper.

The above curve is the epitrochoid, which includes the epicycle as a particular case. By crossing the band we make τ_2 negative, and obtain the equation to the hypotrochoid including the hypocycloid as a particular case.

In this case the curve cannot be reduced to a point, but x may be equal to zero during the motion, or y may be, *i. e.* we may reduce the vibration to either one of two straight lines at right angles to each other.

Prof. CAYLEY mentioned a machine by M. Perigal for describing curves in a somewhat similar manner. Dr Hubert Airy had drawn similar curves with a pendulum.

Mr GLAISHER said that the above machine, exhibited in 1848 at the Royal Society, drew curves of more complexity than that of Mr Ellis. He described the machine and gave a brief sketch of its origin.

March 11, 1872.

The PRESIDENT (PROFESSOR HUMPHRY) in the Chair.

Fellow elected:—

J. W. CARTMELL, M.A. *Christ's College.*

Communications were made to the Society:

(1) *A Monograph of the Ebenaceæ. By* W. P. HIERN, M.A.

The family *Ebenaceæ* was first established by Ventinat in 1799; it was revised by Jussieu in 1804; and in 1810 it was reduced to its present limits by the great botanist Brown.

In 1837, Geo. Don, in his "General System of Gardening and Botany" vol. iv., gave an account of the whole family as understood by him; he enumerated about 80 species which he distributed among 8 genera.

In 1844, Alphonse De Candolle monographed the family in the eighth volume of the "Prodromus systematis Naturalis regni vegetabilis," and produced 160 species and 8 genera. Three of these genera were new and several of Don's genera were not maintained.

In the present monograph 5 genera only are recognized, namely, *Royenà* and *Euclea* from Africa, *Maba* and *Diospyros* from various countries, and *Tetraclis* from Madagascar, the last of which is new; and among these are distributed about 250 species. An account is also given of the fossils that have been published as members of the family, but little confidenee is placed in the determination of the genera or family in the case of the great majority of the fossil species, and they are not included in the above-mentioned estimate.

For the purpose of preparing the present paper, the great collections both in this country and on the Continent have been examined.

The economic properties of the various members of the order are fully described.

The head-quarters of the family is India, where the species are numerous, but of the 5 genera which compose the family only 2 (though these are by far the largest genera) occur in the whole of the East Indian regions. Two genera are peculiar to the continent of Africa, and one, a new genus, is peculiar to the island of Madagascar. Not a single species is indigenous to Europe; one however is naturalized in the countries bordering on the Mediterranean Sea; this one species is indigenous to the Steppes-region of Asia and to China and Japan. Tropical Africa, including Natal, has above 40 species; the Kalahari region of South-west Africa south of the tropic and north of the Orange river has 6 species; and the Cape of Good Hope has above 20 species. Australia has about 16 species, none of which occur on the western coast. The Forest region of the Western Continent of Griesbach has only *Diospyros virginiana*, L.; the Prairie region has 2 species: the Californian coast-region none; the Mexican region 8; and the West Indies 6 species. The South American region north of the equator has about a dozen species; the region of equatorial Brazil 9; and the remaining portion of Brazil 14 species. Madagascar has 23 species; the Mascarene Islands 6; the Seychelles 2; Sandwich Islands 2; Fiji Islands 2; and New Caledonia 11 species.

Lists are given, arranged in numerical order, of collections of Ebenaceæ made by the principal botanical travellers.

A chronological list is also given of the published specific names, with references and localities.

The natural orders bearing the closest affinities to *Ebenaceæ* are *Olacineæ*, *Styraceæ*, *Anonaceæ*, *Ternstrœmiaceæ*, *Sapotaceæ* and

Ilicineæ; a plan is given exhibiting the affinities including these families and others which at a greater distance also bear some affinity to *Ebenaceæ.*

A detailed description of the natural order, the genera, and the species forms the chief bulk of the paper.

An alphabetical list of local names of the species, and diagrams for each genus exhibiting the numbers of stamens in each species, conclude the monograph, which is illustrated by several plates.

(2) *The influence of human degenerations on the production of insanity. By* DR BACON.

The object of this paper was to shew that insanity was a result of degeneration in the race, produced by overcrowded dwellings, vitiated air, insufficient nourishment, interbreeding and the like. The author called attention to the circumstances under which Cretinism existed in the Alps and other places; and shewed that insanity in England was most prevalent in those counties where the agricultural labourers were the worst paid. Thus in Wiltshire 1 in every 12 was a pauper, 1 in every 327 insane: but in Westmorland and Cumberland, where the paupers were 1 in 28 and in 24 respectively, the insane were one in 517 and 543. Hence he held that for the diminution of insanity more must be hoped from measures tending to raise the condition of the people, than from any increase of medical skill.

The PRESIDENT remarked on the importance of the communication, and said that as the town population was increasing at the expense of the rural, it was important to ascertain whether there were any signs of mental degeneration accompanying the asserted physical degeneration among them.

Prof. PAGET enquired whether Dr Bacon had detailed facts in the case of one village which he had mentioned, and made some remarks on the physical degeneration in towns, a larger

percentage of recruits being rejected from towns than from country places.

Dr LATHAM asked whether increasing luxury might not also tend to increased insanity, and this be an equal danger with poverty.

Dr BACON said that no doubt there were evils attendant on civilisation, but that he had founded his remarks on statistics; and that these pointed to the poorest counties being the most liable to insanity.

Dr CAMPION said it must be remembered that the more actively disposed persons left the country, so that the feebler, and those most degenerated, remained behind, and that statistics which did not take account of this could not be trusted.

Some further conversation took place upon the same subject.

(3) *Supplement to a table of Bernoulli's numbers. By* J. W. L. GLAISHER, B.A.

April 29, 1872.

The PRESIDENT (PROFESSOR HUMPHRY) in the Chair.

The Treasurer (Dr CAMPION) gave a statement of the Society's accounts for the past year, which had been audited by Mr PIETERS and Mr MAIN. The thanks of the Society were proposed by Prof. LIVEING, seconded by Prof. BABINGTON, and unanimously accorded.

Communications made to the Society:

(1) *On certain effects on light on Portland Stone. By* F. A. PALEY, M.A.

Mr PALEY described the tendency of Portland stone and the oölites, as Bath, Barnack, Ketton, &c., to contract, in different

degrees, blackness by exposure to the air. He shewed some reasons for doubting if this was due simply to the effects of smoke, and shewed that in all cases, but more markedly in the Portland than in other stones, the blackness was either prevented or removed by the incidence of the sun's rays. Many cases of this were adduced from the buildings in the University, and the black and white portions of St Paul's Cathedral were shewn to be referable to the same causes, or apparently to follow the same law. That the black was not due solely to smut or smoke-marks, was inferred from portions scraped from the blackened surfaces being found to be quite unaffected by soap or solution of soda, and presenting a changed appearance under the microscope. It was suggested, as a question of scientific interest, that the potash or phosphate of lime in some of these stones might, in the course of years, undergo some chemical change analogous to oxidisation; at all events, difficulties were pointed out in the common and obvious conclusion, that the blackening of buildings was in all cases due to the effects of smoke alone.

Mr Trotter made some enquiries as to the positions of the blackened surfaces with reference to the channels down which rain might run.

Mr O. Fisher had only seen these blackened surfaces in London and Cambridge; and that the black was removed by slight exfoliation of the stone when frozen. The stone which lay about near the quarry was not blackened. Possibly the "quarry water" might have something to do with it.

Professor Miller thought that some kind of vegetable growth was the chief cause, instancing the black stains common on limestone and dolomite cliffs. The red sandstone of Strasburg Cathedral—though apparently not a favourable stone—was covered with vegetation.

Professor Liveing mentioned the blackness on parts of the white marble in the floor of King's College Chapel. This he

thought due to vegetation. The action of heat in drawing outwards various crystallizing substances in the stone might keep parts clean that were exposed to the sun.

Mr BONNEY thought that the dampness or dryness of the stone, where sheltered from or exposed to the sun, was the chief cause; as favourable or unfavourable to both the growth of vegetation and the lodgment and chemical action of soot.

Professor BABINGTON said that it was true there was much vegetation on the stone on the north side of King's Chapel, but he attributed this blackness to smoke.

(2) *On Faye's method of comparing mètres à traits; and an improvement of it suggested by* Professor MILLER, F.R.S.

Diagrams of the instruments were exhibited and described, and a few remarks were afterwards made on them by Professor Maxwell.

(3) *On certain lithodomous burrows in the Carboniferous limestone of Derbyshire. By* T. G. BONNEY, B.D.

The author referred to two previous communications on the same subject, and stated that some doubt having been expressed as to the accuracy of his observations in the most important case described in one of them, he had again visited the same neighbourhood. Not only had he confirmed his previous observations, but he had found a large number of other burrows, which he described, exhibiting a very fine specimen; and he maintained these could not be (as had been said) the work of marine mollusca, as Pholades. It was very improbable that they would have lasted so long in limestone rocks; they were

unlike Pholas burrows in shape; they were in positions where it was wholly impossible that Pholades could burrow, as, for example, driven vertically upwards into overhanging slabs of rock; they were at the bottom of valleys of river erosion, such as Miller's Dale and Tideswell Dale, and in one case on a scarp of rock which he was now convinced was artificial. He had some additional evidence for their being the work of snails, and thought that Helix nemoralis and lapicida as well as H. adspersa made them.

Mr NEVILLE GOODMAN described the Monte Pellegrino (Sicily) where the stone all over the mountain is perforated, in situations where the Pholas could not bore, and in rocks which had probably not been submerged since secondary times. He quite agreed, from what he had seen, that these burrows were the work of snails.

Mr O. FISHER asked whether possibly the lime was needed by the snails.

Professor HUMPHRY thought that the mode of making the hole was mechanical, by the odontophore, rather than by chemical action.

Mr O. FISHER exhibited a flint flake from Crayford, which was taken from the old brick earth; it was associated with remains of E. Antiquus and R. Megarhinus, below beds with Cyrena fluminalis and Unio littoralis.

May 13, 1872.

The PRESIDENT (PROFESSOR HUMPHRY) in the Chair.

New Fellows elected:

G. F. SAMS, M.A. } *St Peter's College.*
A. DEY, B.A. }

Communications made to the Society:

(1) *On a method proposed by M. Fizeau for comparing a mètre à bouts with a mètre à traits. By* Professor MILLER.

(2) *On the section exposed at Roslyn Hill Pit, Ely. By* T. G. BONNEY, B.D.

The author stated that hitherto two hypotheses had been proposed to account for the extraordinary collocation of Boulder clay, Cretaceous beds and Kimeridge clay in this pit; (1) which had been advocated by Mr H. G. Seeley and others, that this was the result of faulting; (2) that, as had been suggested by Mr O. Fisher, the cretaceous beds were a boulder-like mass, that had been dropped in boulder clay times from an iceberg into a depression which it had excavated in the Kimeridge clay. He stated that during the last three years he had frequently visited the pit with a view of testing these theories. He pointed out that if the collocation were the result of a fault we should have in the space of about a hundred yards two corresponding down-throw faults bringing down the boulder clay, and an inner pair of (relatively) up-throw faults for the cretaceous beds, which latter were reversed faults. He also shewed that the lower greensand at the E. end of the pit was not, as had been supposed, *in situ*, and that the boulder clay at the S.E. corner formed a wedge-like mass that ultimately disappeared, allowing the gault to come in contact with the Kimeridge clay. He exhibited plans and sections, and argued that the collocation was in the highest degree improbable on a theory of faulting. There was a third hypothesis possible, that the cretaceous beds had slipped from above the Kimeridge clay into their present position, but though some appearances favoured that, he thought it, on the whole, less probable than the

boulder hypothesis; he only differed from Mr Fisher in thinking that the valley existed before the iceberg came. He quoted some instances of large included boulder-like masses, especially one recorded by Professor Morris, in Lincolnshire.

Mr O. FISHER expressed his pleasure at the corroboration which his hypothesis had received. He thought the valley could hardly have existed before, because the clay would have formed sides sloping more than the limits of the Kimeridge clay appeared to do. He had, since writing his paper, sometimes thought that the boulder might have been dropped on the top of the Kimeridge clay and crushed its way down into its present position.

Mr BONNEY, in reply, gave reasons for the supposed pre-existence of the valley, and thought it doubtful whether the bouldor would be heavy enough to crush out the beds below.

May 27, 1872.

The VICE-PRESIDENT (PROFESSOR BABINGTON) in the Chair.

Communications made to the Society:

(1) *On some properties of Bernoulli's numbers, and, in particular, on Clausen's Theorem respecting the fractional parts of those numbers. By* Professor J. C. ADAMS, F.R.S.

The author stated that the theorem enunciated by Clausen for the determination of Bernoulli's numbers had not been proved by him or by any other mathematician—the memoir proposed by Clausen not having ever been published. The author gave a comparatively simple proof of the theorem. Thirty-one of Bernoulli's numbers are already known; the

author has calculated 22 additional numbers. He also had proved that if n were a prime number other than 2 or 3, the numerator of the n in Bernoulli's number was divisible by n.

Professor CAYLEY called attention to one or two points connected with the paper.

Mr GLAISHER said he had observed that in dividing

$$\frac{B_n}{2} \text{ and } \frac{B_n}{2^n}$$

the period of all the circulation was the same; he had verified this for about 28; he had not yet proved it, but conceived it would follow from Clausen's theorem.

(2) *On some of the symptoms produced by Uræmic poisoning in chronic disease of the kidney.* *By* P. W. LATHAM, M.D.

The object of this paper was to shew that many of the symptoms, as to the mode of production of which in chronic Bright's disease much discussion has hitherto arisen, might reasonably be explained. That the factors involved were:—

(1) The impeded passage of the blood through the minute arteries of the system, caused by excessive contraction and hypertrophy of the muscular walls of these vessels, as has been demonstrated by Dr George Johnson.

(2) The hypertrophy of the heart, developed by the resistance offered to the circulation from the contraction of these small arteries; and

(3) The impoverished state of the blood, which is the necessary accompaniment of the disease.

The author first dwelt upon the occurrence of paroxysmal dyspnœa or asthma, and after discussing the effects which would be produced if the minute branches of the pulmonary artery were suddenly contracted, and the general symptoms and physical signs which would accompany such an event, he shewed by reference to cases recorded by other observers, and from instances which had come under his own observation, that the theory was supported by facts. He next referred to epileptiform convulsions and uræmic coma, and pointed out why, in some cases, convulsions might occur, and not in others; owing to the predominance of one or other of the above-mentioned factors. He then went on to say, that, although cerebral apoplexy not unfrequently occurred in chronic Bright's disease, where there was atheromatous degeneration of the arteries; yet that, independently of this, the apoplexy might be caused by the velocity of the blood through the minute tubes being retarded, (the velocity through a tube varying as the square of the radius of the section,) and so leading to the formation of a small coagulum of fibrin or a thrombosis. There would then be complete obstruction, and consequently the greatest possible pressure would be brought to bear on the arterial wall and result very probably in rupture. This also, he contended, explained the production of pulmonary apoplexy, and minute apoplexies in the kidneys and spleen, or hæmorrhagic infractions occurring in chronic Bright's disease, where no valvular mischief of the heart or endocardiac disease existed.

Dr Bradbury thought the symptoms mentioned by Dr Latham were explicable on the supposition that after Bright's disease had set in, thrombosis of the heart had taken place. He described a case of pulmonary apoplexy which he had recently examined, where a large blocking had been caused in the pulmonary artery, and commented upon one or two points in the paper.

Dr Latham thought the condition found *post mortem* in the

case quoted by Dr Bradbury supported the theory he had advanced, for as there was no valvular disease of the heart, the obstruction had most probably been caused by some of the minute branches of the pulmonary artery contracting, so as to retard the velocity of the blood through them to such an extent as to allow it to coagulate.

(PART XIV.)

PROCEEDINGS

OF THE

Cambridge Philosophical Society.

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Cambridge:
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AT THE UNIVERSITY PRESS.

ANNUAL GENERAL MEETING, OCTOBER 28, 1872.

The PRESIDENT (PROFESSOR HUMPHRY) in the Chair.

The following officers were elected:

President.

Professor HUMPHRY.

Vice-Presidents.

Professor CAYLEY.
Professor ADAMS.
Professor LIVEING.

Treasurer.

Dr CAMPION.

Secretaries.

Mr BONNEY.
Mr J. W. CLARK.
Mr TROTTER.

New Members of the Council.

Professor BABINGTON.
Professor STOKES.
Mr HORT.
Mr M. FOSTER.

Communications made to the Society:

On the form suggested by M. Tresca, and adopted by the Commission Internationale du système métrique, for the Mètres Internationaux. By Prof. MILLER.

The instrument was described.

(1) *On Methods of drawing in Perspective. By* Mr J. C. W. ELLIS.

(2) *On a Method of Levelling* (*communicated by* Mr ELLIS) *proposed by* Mr W. H. STANLEY.

1. *On methods of drawing in Perspective.*

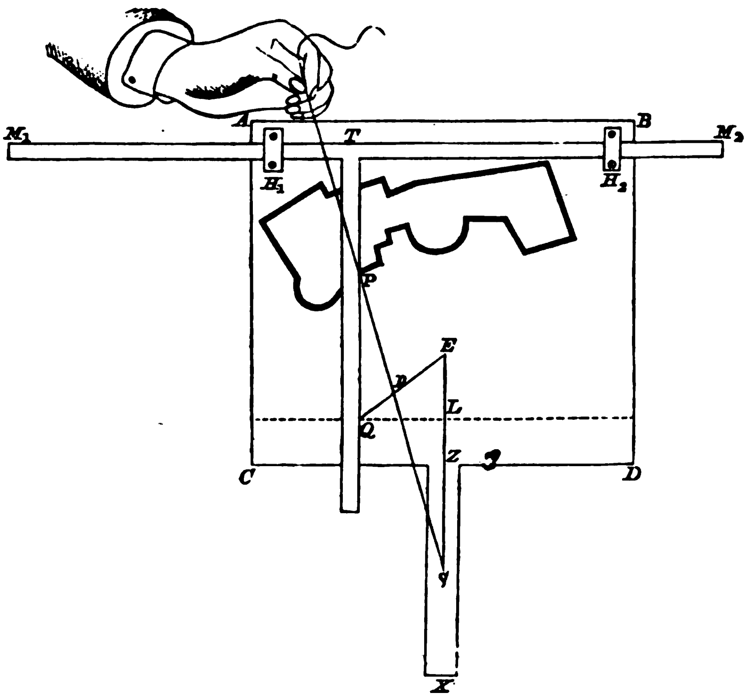
The problem attempted to be solved was this: 'Given the Plan of a building drawn to scale and in any given position with regard to the eye, to cause a pencil by a mechanical arrangement to trace out the corresponding Perspective of the Plan, whilst the operator causes another pencil to follow the outline of the Plan.'

The Mechanical difficulties due to friction, jamming &c. were such, that no satisfactory result was obtainable by the methods employed.

The following step-by-step methods might however be of practical utility, especially in the case of complicated curves.

The Perspective of any object with regard to any given position of the eye is obtained by joining the eye with every point of the object and cutting the cone so formed by a vertical plane. We draw to scale the Plan (whose perspective we require) on a horizontal plane. We assume the eye to be at a height EL above this plane. We take any vertical plane between the plan and the eye, at a perpendicular distance SE from the eye, so that if S be the position of the eye, E is its orthogonal projection on the vertical plane.

ABCD is a Drawing-Board, *ZSX* a bar of wood fixed to the centre of *CD* and flush with the Board $M_1H_1TH_2M_2$ a straight bar sliding on the Board through the fixed guides H_1, H_2, so that *TQ* a ruler, fixed at right angles to M_1M_2 at its central point *T*, sweeps over the Board and is always parallel to the edges *AC* or *BD*.



S is a small ring fixed at any required point in *ZX*. *E* is another, fixed at any required point in the Board. Any point *Q* in the ruler will trace out a line (as *QL*) parallel to *CD*. *ELS* is perpendicular to *QL*. *EL* (drawn to scale) is the height of the eye above the given Plan, *ES* the distance from the plane of reference.

One end of a fine thread is fixed at Q, passes through the rings E, S, and is tightened by the hand so as to pass through the point P, where the ruler TQ meets the given Plan. The point p, where the portions of the thread QE, PS intersect, is the 'perspective' of P, or rather of P' the point in the actual Plan to which P corresponds.

That p is the 'perspective' of P, or the point of intersection of the straight line joining S and P made by a vertical plane at a distance ES from the eye, can be seen thus. To obtain the actual position of the eye, we must draw a straight line ES' from E towards us and perpendicular to the paper so that $ES'=ES$. S' will then be the actual position of the eye. Again, the actual position of P (on the assumed scale) is obtained by drawing QP' perpendicular to the paper and on the other side, so that $QP'=QP$. Now if we join $S'P'$, the point when this line meets the paper is the perspective of P'. This point manifestly lies in the intersection of the paper with a plane containing the parallels QP', ES', *i.e.* it lies in the straight line EQ, and divides EQ in the ratio of ES' to QP', p fulfils these conditions, and is therefore the perspective of P' required.

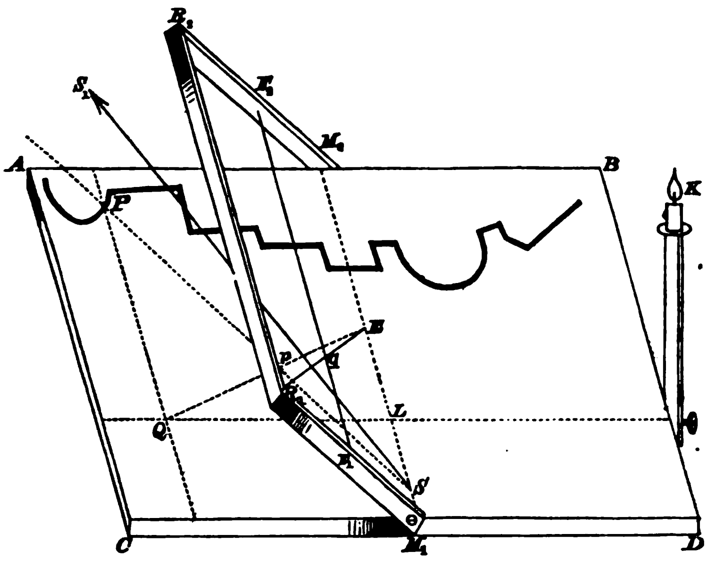
By sliding the ruler, the perspective of every point in the plan, however complicated, may be arrived at; or, in other words, the section of any cone with any vertex and any base may be obtained.

To determine the perspective of any point in an elevation, say n feet above P, draw a straight line through p parallel to the ruler, and where this cuts the thread from S through a point corresponding by scale (as marked off on the ruler) to n feet above P, is the required perspective of the point.

There is a little difficulty, especially in some positions, in marking accurately the point p with a pencil however fine, owing to the effect of Parallax, as both threads cannot lie exactly in the plane of the Board, and also because they must be slightly pushed aside in order to mark with the pencil.

These difficulties may be overcome by a method of shadows in the following manner.

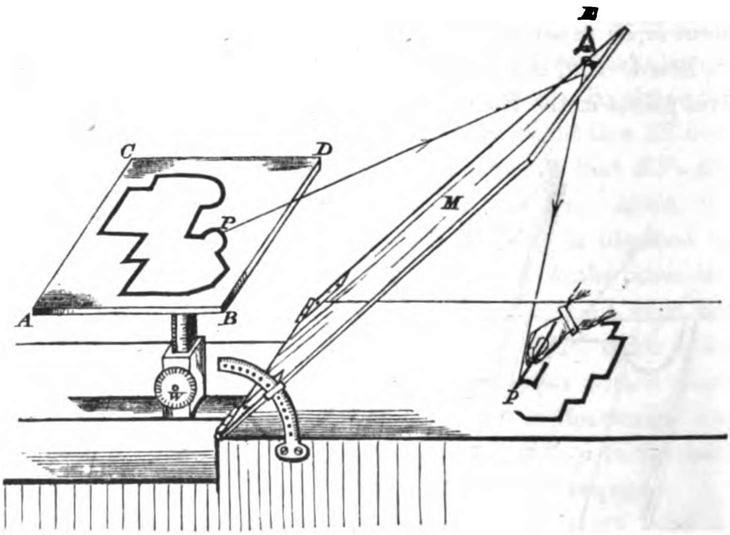
Two rods R_1M_1, R_2M_2 are screwed into the middle points of the opposite sides CD, AB of the Board, and their ends are connected with the rod R_1R_2 so as to form the three sides of a parallelogram. The plane of this parallelogram by revolving about M_1M_2 as an axis may make any angle with the plane of the Board. F_1F_2 is a thread stretched parallel to R_1R_2. E, S are fixed points in the Board in the straight line M_1M_2.



The thread R_1E meets F_1F_2 in q; SS_1 is a thread tightened by hand. K is a candle placed in a vertical plane through q. The shadow of q, namely Q, will trace out a straight line, QL, as in the first method parallel to CD. The shadow of F_1F_2 will be QP parallel to AC, and the intersection of the shadows of R_1E and SS_1, *i.e.* p will be the required Perspective of P.

Another method is represented below. The Board is supported upon a stand which may be slid to any position by run-

ning it in the horizontal groove. It may be raised to any required height by turning the milled head *W*, and the height is read off on a scale. *M* is a sheet of transparent glass, which is hinged at its lower end, and may be fixed at any required angle with the horizontal plane. In ordinary perspective this angle



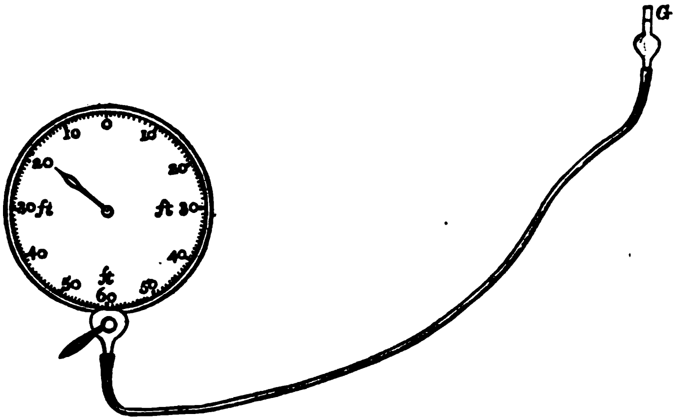
is 45°. The eye *E* on looking through a small hole in an upright attached to the upper edge of the glass sees by reflexion any point *P* of the Plan transferred to *p*, which is the perspective of *P* corresponding to the eye in the position *E* with regard to the given Plan. To obtain the elevation of the building, say at a height of 20 feet, we have only got to elevate the Plan by turning the milled head *W*.

The Perspective of the Plan at any elevation *above* the eye may be obtained by drawing the Plan for the same elevation *below* the eye, and then transposing it, by copying it through the sheet of glass held vertical.

It is manifest by giving various inclinations to the glass we may obtain the corresponding plane sections of a cone of any form.

2. *On a Method of Levelling.*

A gauge, similar to a steam-gauge, is attached to a fine india-rubber pipe 22 yds. long. At the other end is a glass



tube a few inches long with a fixed mark upon it, and just below the glass tube the pipe expands into a small india-rubber bulb. The observer holds the gauge and advances 22 yds., his assistant remains behind and holds the other extremity of the tube, the tube being filled with water. The gauge indicates the pressure due to the difference of Level. By squeezing the bulb the water can always be brought up to the fixed mark in the glass tube *G*. The observer and his assistant always hold the gauge and the glass tube, say at the height of the eye in each observation. Each lb. of pressure corresponds to a difference of Level of nearly 2 ft. The number of feet corresponding to the pressures are marked upon the gauge, from 0 up to + 60 ft. on the right hand and down to − 60 ft. on the left hand. In the figure the gauge points to + 20, indicating that the observer is 20 ft. below the position of his assistant; had it been − 20, he would have been 20 ft. above him.

As soon as the observer has entered this + 20, in his field-book, the assistant comes on and stands where the observer did, whilst the observer goes on and takes another observation. In order to determine the difference of Level between any two stations he has only to take the algebraical sum of the No. of feet recorded in his field-book.

Instead of the glass tube, a second gauge might be employed, and the *difference of readings* of the gauges entered as the differences of level at each step. In this case the gauges need not be constructed to read below zero.

This method of Levelling would be extremely rapid and sufficiently accurate for most practical purposes. The instrument would be inexpensive, and could be used by an uneducated observer. It could also be employed at night, which would render it useful in military work before an enemy.

By using mercury instead of water, and a very fine flexible tube of copper wire, the readings might be rendered extremely accurate, and the whole weight of the instrument reduced to a very few lbs.

Professor LIVEING communicated a note on Mr Paley's paper, "On effects of light on Portland stone," read at a former meeting. During the summer he had visited Portland, and had found that all the stone there became black on all old exposed surfaces. He had sent specimens of the black part to Mr Berkeley, from the Portland quarries and from King's College Chapel; who said that they were the early stage of some lichen. Professor Liveing had found this blackness on St George's Church on the top of Portland, but only on surfaces exposed to rain drip—the tombstones also were discoloured. Mr Berkeley had informed him that the lichens in the two cases named above were not the same species. The stones at Portland also occasionally had a red lichen growing on them.

New Fellow elected: H. W. WILSON, Hon. M.A.

November 11, 1872.

The PRESIDENT (PROFESSOR HUMPHRY) in the Chair.

Communications were made to the Society:

(1) *On the advantages of Denison's Gravity Escapement for recording time by Electricity. By* Mr W. KINGSLEY.

The effects of corrosion and irregular impulse are avoided in this escapement by making the current pass through a portion of the pendulum and gravity arm; the pivot on which the pendulum is hung, the gravity arms and the banking pins being insulated, and the terminals of the battery being connected one with the top of the pendulum and the other with the banking pins, the current is made and broken at the end of each impulse; the wear and tear being confined to unimportant parts.

(2) *Description of a form of Remontoir Clock invented by M. Groux. By* Mr W. KINGSLEY.

The principle of this remontoir is the making the fly fan of the remontoir lock on an intermediate detent, so that the usual concussion and friction are avoided.

Professor MAXWELL commented with approval on the principle of the remontoir clock, mentioning one or two conspicuous defects in existing clocks, and alluding to the difficulty under which a clockmaker laboured in devising any improvement on existing patterns.

(3) *On certain facts connected with the wasting and final disappearance of the Glaciers of North Wales. By* Mr W. KINGSLEY.

The object of this paper was to shew that the Glaciers of North Wales were much larger than is commonly supposed,

and more like those of Greenland than of the Alps; filling, as they did, extensive basins, and having little motion excepting at certain points of escape.

That these icefields during their decrease dropped their moraines over large slopes and the hill-sides in such a way as to make it difficult to distinguish the moraine deposit from drift.

Thirdly, shewing in certain cases the manner in which moraines were deposited as the glaciers broke up into smaller ones in the higher recesses of the mountains.

Fourthly, drawing attention to the marks left as evidence of great floods in the valleys during the periods of the wasting of the ice.

Lastly, giving an account of large deposits of fresh-water diatoms in the lakes; these deposits having been made since the glaciers disappeared, but during a cold epoch; and proving that no sea had reached these lakes since that epoch, but giving a means of estimating the time that has elapsed since the glaciers thawed. The whole paper was intended to draw attention to these facts in order that persons might be induced to pursue these investigations to a much greater extent than has hitherto been done.

Professor C. C. Babington spoke very highly of the value of Mr Kingsley's paper, which, he said, had explained to him several things which he had always found much difficulty in understanding.

Mr Bonney said that Mr Kingsley's paper was a most interesting one, dealing with a particular case of a general problem, the condition of the northern hemisphere in the glacial period. The author appeared to him to have proved that in the earlier part of that period Wales, like Scotland and Scandinavia, had been covered by an ice-sheet—differing thus from Switzerland, where separate glaciers seemed rather to have existed. He was disposed to refer the drift-like scattered

moraine matter, the like of which he had seen in the Alps, not to the retreat of the glaciers after the above period, but to their retreat when, after the great submergence following the ice-sheet period, *glaciers* had formed in the valleys. The enlarged river-channels mentioned by Mr Kingsley were, in his opinion, not due to floods in the ordinary sense of the word, but to the rivers themselves having once been much greater than now, as they might be expected to have been towards the end of the glacial epoch. He had investigated these large river channels in most districts of Great Britain, and in a considerable part of Northern and Central Europe.

Mr O. FISHER was disposed to think that evidence of the ice-sheet period might be obtained even in so flat a country as Cambridgeshire, in certain singular contortions and disturbances of the drift, and superficial deposits, which he could only explain by the pressure of a great mass of ice. He thought that the absence of shells in many of the drifts rendered it unlikely that they were marine, and was disposed to consider that the numerous ice-marks which he had seen in the East of England belonged to an earlier period than those in the West.

New Fellow elected: J. B. LEE, B.A., Sidney College.

November 25, 1872.

The PRESIDENT (PROFESSOR HUMPHRY) in the Chair.

Communications were made to the Society:

(1) *On the appearance of an extra digit on the hind limbs and then on both fore and hind limbs in two successive generations; and its bearing on the theory of Pangenesis. By* Mr N. GOODMAN.

The facts which had come within his personal knowledge, and on which he submitted some remarks, were the following:

Mr Daintree, of Fenton, Huntingdonshire, bought a cow with three well-developed toes on each hind limb besides the two ordinary rudiments which hang behind the foot. This cow was without a pedigree or history. She had a cow calf with the same peculiarity as its dam, which was as well developed as in her case, notwithstanding that the other parent was a normal bull. This cow has had two calves by normal bulls. The first was a cow calf with three toes on each hind limb, but somewhat less developed and less functionally insistent on the ground than in the case of its mother and grandmother; the second was a bull calf, which had three toes on all four feet. All the toes assumed to be the extra ones have a similar attachment, viz. on the inside of the foot between the internal functional toe and the rudimentary toe on the same side.

Mr Goodman gave a short account of Mr Charles Darwin's theory of Pangenesis, whose main feature is that each individual is made up of organic units, all of which are constantly giving off minute gemmules which float freely through the organism and are transmitted in the generation products to the next offspring, and which are so much in excess of what are required for the building up of the body of the immediate progeny, as to be handed down, many of them, through a great many generations in a latent undeveloped condition. He then applied this theory to explain the facts.

The extra toe might be due, in accordance with this theory, to one or more of the following causes:—

I. Atavism or reversion to an ancestral type.

II. The modification of the proliferous function of certain of the organic units produced by external causes.

III. Correlation of growth, supplementing the last-named cause.

He argued that the abnormality was not due to atavism—

(1) Because it was necessary to travel so far away from the species before a three-toed ungulate was found.

(2) Because the toe did not appear in the position where it must have appeared, on this hypothesis, if received homologies of the toes were reliable.

(3) Because precisely analogous cases had been found in the human subject. Six-fingered and six-toed men were not very uncommon; and as no beast, bird, or reptile had more than five digits on each limb, and yet the extra fingers and toes were definitely human, this evidence was conclusive against the abnormality being due to atavism.

The second cause might operate in two ways. The organic units might be stimulated to throw off more gemmules, or these gemmules might have enhanced affinities. In either case they might attach themselves to any nearly allied growing part between which and themselves there was an affinity resembling their natural one, and so by effecting a double attachment give rise to a double organ. Nor would the extension of the extra abnormal part to a fresh limb, as in the case of the young bull, be unaccountable. For the extra digit, being the direct descendant of a normal one whose organic units had been excessively proliferous, would resemble the parent part not only in structure but in the vigour of its budding function. Thus we should have in the desired animal two energetic manufactures of gemmules instead of one, and in the third generation a still greater excess or avidity in the transmitted gemmules which would manifest itself in a fresh attachment.

But if this were the true explanation of the peculiarity, it would follow that the extra digit, though it had the attachment of a *finger*, would be in reality, as in structure, a *toe*. It is difficult to draw a distinction between the toe and finger of an ox; but as a precisely analogous case had occurred in the human subject, they might safely reason from that. In the case referred to, the extra part had originated in the hands

and been extended to the feet, and in that case the digit was decidedly not a finger but a toe.

They were then driven to the last cause—

III. The correlation of organs. This phrase expressed our ignorance rather than explained it away. Nevertheless some distinctions might be drawn as to the nature of the *correlation*, which was exhibited by the extension of the extra part to the fore limb.

1. It was *not* a teleological correlation, like that which associated the carnivorous teeth with the transverse glenoid cavity for the reception of the hinge of the lower jaw.

2. It was *not* a correlation such as that by which the single occipital condyle was found associated with the segmented jaw in the sauroids.

3. It was not a physiological correlation, since it affected parts containing various structures with various functions—as nerves, bones, &c.

4. It was not an entirely arbitrary (which meant an entirely lawless and unexplained) correlation, as that which determined that white cats with blue eyes should be deaf.

5. It *was* a correlation which obeyed laws and was capable of analogical illustration. It would seem to be due to a force operating in the organism which is best expressed by calling it "polarity," and finds its best analogy in the force which determines that a crystal shall be built up symmetrically around its axes, and that no molecule can be added on one side of these axes without a corresponding one attaching itself to the other. Or perhaps the polarity exhibited in electrolysis affords even a closer analogy. It would be interesting to find that there was latent in the system of the highest vertebrates a force so masked by other dominant forces and exacting conditions as only to appear thus suddenly and abnormally, yet which was identical with that which determines the shape of the simplest forms of inorganic matter.

Mr Goodman, during the reading of the paper, referred to many abnormalities besides the one which formed the subject of his paper, and many of his deductions were derived from these.

The PRESIDENT made some remarks on the theory cf Pangenesis, and read an extract from Darwin, wherein he stated his hypothesis of Pangenesis, and pointed out some of the difficulties attendant upon it, as the difficulty of understanding how such an immense number of gemmules could be contained in a minute ovum, and how they could be transmitted unchanged through successive generations.

Professor MAXWELL spoke of the difficulty of conceiving of chemical molecules in sufficient quantity being packed in these small gemmules.

Dr M. FOSTER spoke of the difficulties in the theory of Pangenesis, especially that mentioned by Prof. Maxwell; still on that point we have limited positive information as to the size of those chemical molecules, nor do we know how many gemmules were required to be contained in an ovum. It was a morphological limit Mr Darwin was really rather seeking—such a one he (Dr Foster) did not think to hold good, rather was there a physiological one. The "cell" did not always exist as in some low forms of Protozoa. We cannot detect a structure in Protoplasm, still there must be some approach to structure. If you go on dividing it there must be a limit of division, beyond which it cannot continue to live; this therefore suggests a physiological limit. He thought that to the "primary" affinities of gemmules secondary affinities must be added, these must influence also its future fate. With regard to this special problem described by Mr Goodman we must consider (1) How this abnormality first made its appearance; this be regarded as a case of re-duplication, a tendency to which as to fusion was well-known: this he thought did not disagree with Pangenesis, but that the secondary affinities were

acted upon. (2) How it was reproduced in the offspring: he thought that the transference of the digit could be explained as Darwin had done, by the correlation of homologous parts; he thought that these changes were a strong confirmation of the doctrine of homology. He preferred then to regard this result as coming from the action of secondary affinities (he objected to the term polarity) causing re-duplication, and from the homologies of the members.

Prof. Paget asked how on Darwin's hypothesis it happened that parental defects were often not transmitted.

Dr Campion asked whether there was a tendency to reduplicate on the outside, especially in the thumb of the hand and the little toe.

Mr Goodman, in reply, said that he had, in a portion of his paper which he had omitted for curtailment, called attention to the action of primary affinities which determined the association of the gemmules in the generative products, and the secondary affinities by which they were built up in the derived organism; he shewed that according to Mr Darwin when results were produced by defect of gemmules this could be made up in the next generation by fission of the gemmules. He shewed from instances that the increase was not always on the inside. He gave also some explanation of the muscular system of the abnormality.

Prof. Paget objected that defects were often made up while excesses were very rarely, so that the chances seemed against the explanation offered by Mr Goodman.

Mr Goodman pointed out that it was not only the gemmules from the last ancestors that were transmitted, but from many previous ancestors.

Some further conversation took place on the subject of Pangenesis.

(2) *A Pneumatical Design for saving life at sea.* By Mr W. M. Stanley. (Communicated by Mr J. C. W. Ellis.)

Reservoirs of condensed air communicate by means of pipes (similar to gas-pipes) laid throughout the ship. These pipes serve to lead the air into large flexible balloon-like bags stowed away against the ceilings of the various compartments. A handle being turned on deck allows the condensed air to escape from the reservoirs and to expand the bags. Hence in such a case as that of the "London," where the waves filled the vessel, or in the case of a severe leak, the water would be expelled as no pumps could expel it, and a great additional buoyancy given to the vessel. Again, in the case of a fire, the vessel could be partly submerged by opening valves, and the water again driven out by turning on the condensed air.

A better method than that of condensed air would be, perhaps, were a reservoir of water used saturated with ammonia. A steampipe leading to this reservoir would cause the water instantly to part with many times its volume of ammonia and to fill the bags. Or some gas, such as carbon dioxide, in a liquid form, might be employed.

February 3, 1873.

The President (Professor Humphry) in the Chair.

A special general meeting of the Cambridge Philosophical Society was held, when the following alterations were made in the bye-laws:—

To substitute in bye-laws, Sec. vi. § 2 (hour of meeting), *half-past eight* for "half-past seven," and *half-past ten* for "half-past nine."

To make the following new bye-law—

Residents in Cambridge or the neighbourhood, not being graduates, may be elected Associates of the Society. Each one shall be proposed by three Fellows of the Society, nominated by the Council, and elected by the Society. An Associate shall be elected for a period of three years, and if not then a graduate, shall be eligible for re-election. Associates shall have the privilege of attending the meetings and consulting the books in the Library of the Society.

After an eloquent address from the President on the loss sustained by the death of Professor Sedgwick, whom he justly described as really the founder as well as the ardent promoter of the Society, it was moved by Professor MILLER, and seconded by Professor LIVEING, that an expression of the deep regret felt by the Society at the loss of Professor Sedgwick be recorded on the minutes.

The following communications were made:—

On the Proof of the Equations of Motion of a connected system. By Prof. CLERK MAXWELL.

To deduce from the known motions of a system the forces which act on it is the primary aim of the science of Dynamics. The calculation of the motion when the forces are known, though a more difficult operation, is not so important, nor so capable of application to the analytical method of physical science.

The expressions for the forces which act on the system in terms of the motion of the system were first given by Lagrange in the fourth section of the second part of his *Mécanique Analytique.* Lagrange's investigation may be regarded from a mathematical point of view as a method of reducing the dynamical equations, of which there are originally three for every

particle of the system, to a number equal to that of the degrees of freedom of the system. In other words it is a method of eliminating certain quantities called reactions from the equations.

The aim of Lagrange was, as he tells us himself, to bring dynamics under the power of the calculus, and therefore he had to express dynamical relations in terms of the corresponding relations of numerical quantities.

In the present day it is necessary for physical enquirers to obtain clear ideas in dynamics that they may be able to study dynamical theories of the physical sciences. We must therefore avail ourselves of the labours of the mathematician, and selecting from his symbols those which correspond to conceivable physical quantities, we must retranslate them into the language of dynamics.

In this way our words will call up the mental image, not of certain operations of the calculus, but of certain characteristics of the motion of bodies.

The nomenclature of dynamics has been greatly developed by those who in recent times have expounded the doctrine of the Conservation of Energy, and it will be seen that most of the following statement is suggested by the investigations in Thomson and Tait's *Natural Philosophy*, especially the method of beginning with the case of impulsive forces.

I have applied this method in such a way as to get rid of the explicit consideration of the motion of any part of the system except the co-ordinates or variables on which the motion of the whole depends. It is important to the student to be able to trace the way in which the motion of each part is determined by that of the variables, but I think it desirable that the final equations should be obtained independently of this process. That this can be done is evident from the fact that the symbols by which the dependence of the motion of the parts on that of the variables was expressed, are not found in the final equations.

The whole theory of the equations of motion is no doubt familiar to mathematicians. It ought to be so, for it is the most important part of their science in its application to matter. But the importance of these equations does not depend on their being useful in solving problems in dynamics. A higher function which they must discharge is that of presenting to the mind in the clearest and most general form the fundamental principles of dynamical reasoning.

In forming dynamical theories of the physical sciences, it has been a too frequent practice to invent a particular dynamical hypothesis and then by means of the equations of motion to deduce certain results. The agreement of these results with real phenomena has been supposed to furnish a certain amount of evidence in favour of the hypothesis.

The true method of physical reasoning is to begin with the phenomena and to deduce the forces from them by a direct application of the equations of motion. The difficulty of doing so has hitherto been that we arrive, at least during the first stages of the investigation, at results which are so indefinite that we have no terms sufficiently general to express them without introducing some notion not strictly deducible from our premisses.

It is therefore very desirable that men of science should invent some method of statement by which ideas, precise so far as they go, may be conveyed to the mind, and yet sufficiently general to avoid the introduction of unwarrantable details.

For instance, such a method of statement is greatly needed in order to express exactly what is known about the undulatory theory of light.

(2) *On a problem in the Calculus of Variations in which the solution is discontinuous. By* Prof. CLERK MAXWELL.

The rider on the third question in the Senate-House paper

of Wednesday, January 15, $1\frac{1}{2}$ to 4, was set as an example of discontinuity introduced into a problem in a way somewhat different, I think, from any of those discussed in Mr Todhunter's essay[1]. In some of Mr Todhunter's cases the discontinuity was involved or its possibility implied in the statement of the problem, as when a curve is precluded from transgressing the boundary of a given region, or where its curvature must not be negative. In the case of figures of revolution considered as generated by a plane curve revolving about a line in its plane, this forms a boundary of the region within which the curve must lie, and therefore often forms part of the curve required for the solution.

In the problem now before us there is no discontinuity in the statement, and it is introduced into the problem by the continuous change of the co-efficients of a certain equation as we pass along the curve. At a certain point the two roots of this equation which satisfy the minimum condition coalesce with each other and with a maximum root. Beyond this point the root which formerly indicated a maximum indicates a minimum, and the other two roots become impossible.

New Fellow elected: A. FREEMAN, M.A., St John's College.

February 17, 1873.

The PRESIDENT (PROFESSOR HUMPHRY) in the Chair.

(1) *On the name "Odusseus" signifying "setting sun," and the Odyssey as a Solar Myth. By* Mr PALEY.

This shewed that the name of *Odysseus* or *Ulysses* was more probably connected with *δυόμενος ἥλιος*, "setting sun," than with *ὀλίγος*, "dwarf." It was shewn that all the details

[1] *Researches in the Calculus of Variations, &c.*

of the Odyssey were easily interpreted as connected parts of a solar myth, describing the journey of the sun to the west, and his return, after many struggles and adventures, to his ever-young bride in the east, *Penelope*, the "spinstress," *i.e.* cloud-weaver. The general geography of the Odyssey was noticed, as pertaining rather to Magna Græcia, while the Iliad is essentially Asiatic in its scenery and description. The Cyclops was shewn to be the Sun's eye, extinguished by Ulysses, *i.e.* lost by the Sun when he sinks into the west. The sorceress Circe, and the nymph Calypso, "the coverer," were interpreted as exercising that weird influence over the Sun that is still, by rude races, attributed to magic or to the evil eye. The guidance of Athena, the goddess of dawn, was shewn to be a fitting companion and guide to the Sun in his return. The wreck of Ulysses, and his narrow escape from drowning, was shewn to represent the Sun sinking in the western ocean. Finally, the killing of the suitors with the bow was shewn to be consonant to the usual representation of Apollo and Diana, the sun-god and moon-goddess, who were thought to slay mortals with their deadly arrows. The old Laertes, the father of Ulysses, was compared with old Tithonus, the bride of Aurora, and the symbolism explained by the union of the ever old with the ever new.

Prof. SELWYN remarked with reference to the small stature of Odysseus that the sun became larger on approaching the horizon. A passage, however, in the eleventh book shewed a connexion with a solar myth, where Circe tells Odysseus he will feed the herds and flocks of the sun, which were tended by two shepherds (Mercury and Venus). Also a passage in the Iliad, where Jupiter says that all the gods together could not draw him from his seat, but he could lift them with his left hand, the centre of gravity of the system being within the sun.

Mr PALEY said that loss of strength might be meant, and that Plato had referred the myth above named to a solar origin.

Mr JEBB asked (1) how far the allegory was conscious or

unconscious. (2) Concerning the etymology. He thought that the first idea of a great journey might be taken from that of the sun, but he doubted whether the human interest, introduced into the Odyssey, allowed us to bring it into the same category, except so far as that one archetypal idea might underlie it. Therefore he doubted whether the allegory was at all present to the mind of the writer; and so he thought that the journey and its incidents were essentially human; the framework indeed might be supposed suggested by the solar journey, but in the incidents the writer was dwelling on the human side, and so he doubted whether it could be called a solar myth. (3) As to the etymology, the derivation from *ὀδύσσετο*, as used by Ino, "he with whom the gods were angry," was generally accepted in the best and most critical times of antiquity; and still has the sanction of Curtius, who refers it to *ὀδύσσομαι*, explaining the *ὀ* as prosthetic (of *ὀβελοι*, &c.), thus it would mean the wrathful one, and express the majesty of anger.

Mr PALEY said he thought that the author viewed Odysses simply as a man, but unconsciously followed the tradition.

Mr JEBB asked how far Mr Paley regarded the details, *e.g.* those concerning Circe, Calypso, as supplied by the earlier allegory, or as arising from the mind of the poet when writing on a very simple framework.

Mr PALEY said it was very difficult to say, but he thought that the vitality of the myth would affect them.

Mr FENNELL thought the human interests attached to a myth would cause a very anthropomorphic form to be given to it; and so a hero would grow out of the myth, and action be grouped around him, in accordance with a tendency common in ancient times. He thought Curtius might be wrong about the prosthetic omicron, and that it might be a relic of an old preposition, which still remained in Sanscrit.

(2) *On the identity of the modern Hindu with the ancient Greek ship.* (A model of a Bengalee ship was exhibited.)

This communication explained, by reference to the model of a Hindu (Bengalee) boat, the minute identity in all the details of the mast, sails, tackle, and rudder (or stern-paddle) between the old Greek ship and the modern Indian river-boat. The lowering of the mast, the working of the yard-arm by a man who watches and co-operates with the steersman, and the manner of bringing to shore and fastening the boat, stern ashore and prow to the sea, were illustrated by quotations from Greek poets. The mechanism of the rudder, or paddle, with its "rudder-bands," was explained, with the motion of its own axis produced by the tiller or handle, *οἴαξ*, the rudder itself being called *πηδάλιον*. It is a large and heavy timber requiring many men to lift and carry it. Many technical terms in the Greek writers were identified and explained by the model, which appeared to represent the unchanged model that has prevailed for above 2000 years.

Professor MILLER said that the boats on the Boden See were exact models of ancient boats, and described some peculiarity of their rudders.

Professor LIVEING said, with reference to a point Mr Paley had discussed, that he believed the larger junks in the East still had two rudders.

Mr PEARSON (Emmanuel) thought that at any rate in classical times the rudder had been doubled.

March 3, 1873.

The VICE-PRESIDENT (PROFESSOR ADAMS) in the Chair.

Notes on the Hippopotamus. By Mr J. W. CLARK.

Mr J. W. Clark exhibited the mounted skeleton, and some portions of the visceral anatomy (preserved in spirit) of the young female hippopotamus, which was born in the Gardens of the London Zoological Society, on January 7, 1872, and died on the following Wednesday. The remarks he made in illustration of the specimen were in substance a *résumé* of the paper read by him before the Zoological Society on Feb. 20, 1872, and printed in their Proceedings.

On the Foraminifera and Sponges of the Cambridge Upper Green Sand. By Mr W. J. SOLLAS.

After a description of the Green Sand, and an enumeration of its characteristic foraminifera, the author discussed the origin of its abundant green grains, and indicated that to a large extent these bodies consist of the casts of foraminifera. The included coprolites of the formation were next investigated. Their marked connection with previously existing organic matter was noticed, and it was shewn that this connection characterised the coprolites of various other deposits. Hence was derived a definition for the word "coprolite:" coprolites being defined as "those bodies which have been produced by the phosphatic fossilisation of organic matter, or of the immediate products of its decomposition." The nature of the organisms which furnished this organic matter was shewn, in the case of nodules of hitherto obscure origin, to be spongeous. The sponge-like form of these nodules, the characters and arrangement of their well-marked oscules, and the forms and

disposition of their siliceous spicules, seemed to leave no doubt upon this point. A comparison was instituted between the coprolites of the Green Sand and those of other formations; in the Lower Silurian of Canada, sponge-like coprolites had been met with. The chalk flints only differed from coprolites in being silicified instead of phosphatised sponges. Ventriculites accompanied both, and in both xanthidia and foraminifera were found. The phosphate of lime which fossilised the green sand sponges might have been derived from the volcanic rocks of Lammermuir, and conveyed to them by the cold current which afterwards eroded the gault and supplied the silicates to infiltrate the foraminiferal casts. Finally, the formation of coprolites appeared to be proceeding in the Chincha Islands at the present day.

Mr PALEY asked how the occurrence of phosphate in Terebratula was to be explained if the coprolites were to be attributed to sponges.

Mr SOLLAS explained that he did not refer all these phosphate nodules to sponges, but to the phosphatization of animal matter.

Professor LIVEING asked what was meant by the nodules being derived from the Gault, for he thought that there was no evidence of the nodules occurring in the Gault.

Mr BONNEY stated that nodules corresponding very closely with those of the Upper Green Sand did occur in the Gault, as for example in the Barnwell pits and in Roslyn pit at Ely. They also occurred high up in the Gault at Folkestone, and were not confined to the base of that formation, although layers existed there, as at Upware and other localities. He congratulated Mr Sollas on the excellent work which he had done with these obscure organisms, and agreed with his results.

Mr PALEY said that the form of the green grains was too regular to make it probable that they were fragments from volcanic rock.

On a Boulder in a Coal Seam, South Staffordshire. *By* Mr BONNEY.

This boulder was found in the 13th coal of the Cannock and Rugeley Colliery—which seam is about three yards thick, and probably about 200 feet above the base of the coal-field, which in South Staffordshire rests on upper Silurian rocks. It weighs 13 lbs. 13½ oz., and is about 19 inches in girth either way, and about 4½ thick. The rock is a very compact grey quartzite, which exactly resembles that of the pebbles in the Bunter conglomerate of Staffordshire. He thought it had been brought entangled in the root of a tree. The difficulty was to find out whence it came. The Bunter pebbles were supposed to have chiefly come from Old Red Sandstone rocks of east Scotland, and to have been originally derived from much older highly altered rocks, probably rather in the north-west of Scotland. The general course of the sediment in both the Bunter and Carboniferous times was from the north-west, and it was probable that the pebble too came from that quarter. The principal difficulty in that supposition was that all the known beds containing similar pebbles to the north-west did not appear likely to have been undergoing denudation in Carboniferous times. Hence the author thought that with our present knowledge the problem could only be stated and not solved.

New Fellows elected:

C. W. MOULE, M.A., Corpus Christi College.
C. W. HITCHINS, B.A., Sidney Sussex College.

MONDAY, *March* 17, 1873.

The VICE-PRESIDENT (Professor LIVEING) in the Chair.

On an improved Camera Lucida invented by Professor Govi of Turin. By Prof. MILLER.

The peculiarity of the instrument consisted in the use of a transparent film of gold leaf, by means of which the view of the object was improved; but a description cannot be given without diagrams.

Professor LIVEING spoke of the advantage of a methylic aldehyd for depositing silver, and enquired why gold was selected for the above process in preference to other metals.

Professor MILLER said it was, he believed, because of the thinness and durability of the film that could be obtained.

Professor CLERK MAXWELL spoke of some of the advantages of using the films of gold, and commented upon the instrument.

The following were elected Fellows of the Society:—

V. H. Stanton, B.A., Trinity College.
A. P. Humphry, B.A., Trinity College.
J. Dew-Smith, B.A., Trinity College.
C. Yule, B.A., St John's College.

The following were elected Associates of the Society:—

Mr J. Carter, Cambridge.
Mr A. Graham, Cambridge.
Dr Bacon, Fulbourn.
Mr W. J. Sollas, St John's College.
Mr H. N. Martin, Christ's College.
Mr T. Bridge, Non-Collegiate Student.
Mr A. J. Jukes-Browne, St John's College.
Mr P. H. Carpenter, Trinity College.
Mr W. Marshall, Ely.
Mr W. E. Pain, Cambridge.
Mr F. M. Balfour, Trinity College.

April 28, 1873.

The VICE-PRESIDENT (Professor LIVEING) in the Chair.

On some so-called "Horite" caves at Beit Jibrín (Eleutheropolis). *By* Prof. PALMER.

Beit Jibrín is the ancient Bethogabra or Eleutheropolis, but the modern name is much older than the Greek appellation and represents the Hebrew Beth Giborím, *the House of Giants*, a name suggestive of the gigantic Philistine inhabitants of Gath: indeed Beit Jibrín is without question the site of Gath, and not only does it fulfil the topographical conditions, but amongst its ruins is one bearing the name KhírbetJát, i. e. the the ruins of Gath.

Here are some curious excavations which nearly all travellers who have visited them assume to be Horite, and of great antiquity. Dr Robinson, in his *Biblical Researches*, says some contain inscriptions which are the work of casual visitors and do not throw any light on the age or object of their construction: as many of them are written on the domed roof at a height of about 30 feet, and in a totally inaccessible position, it is hard to imagine how this could have been the case.

Professor Palmer read some extracts from his diary, written during a visit to the caves in question, in which he stated that:—

The caves at Beit Jibrín are evidently quarries, though afterwards wrought into their present shape with some ulterior object, such as the formation of granaries, stalls for cattle, &c. The stone is much better at the bottom than at the top, and the method pursued in excavating seems to have been to work downwards, leaving a hole in the roof to give light, and smoothing off the walls as they went on. The last touch of smoothness in some of the walls appears to have been given by cutting out little niches or pigeon-holes and then knocking out the

interstices, by which means a good deal of labour was saved. One cavern is completely covered with blocks which have not been removed in this manner—but the walls of others shew frequent traces of them. The caverns are certainly not earlier than the Christian era, as there are numerous crosses and figures and Cufic inscriptions, the last apparently not earlier than the 4th or 5th century. One of these inscriptions is a personal prayer for the writer as one of the labourers in the cave. Nor could these inscriptions have been done at any other time than during the construction of the chamber. That just mentioned for instance is at a height of 30 feet, and on the arch of the domed roof, so that it could not have been written from a ladder (even if such a thing could have been obtained, and even now tall ladders are unknown in native oriental building), for the writer would have been leaning back in an impossible position, and had to stretch out and carve with mason's tools a distance of 3 or 4 feet on either side. Nor could a scaffold have been used, as it would have been impossible to sling it from the walls, and it must therefore have been built up from the ground, which is absurd for such a mere private and idle inscription. The figures and some of the crosses, especially the geometrical figure (which is done with mason's compasses), imply the same difficulties and, as well as the inscription just mentioned, would require hours of idle work; just such as might have been done while the steps were still remaining. The inscriptions on the unfinished and finished parts are of the same date and character. The inscriptions consist of formulæ like those of Islam, but without mention of Mohammed; there is, however, no reason to suppose that the well-known formulæ of Mohammed were other than borrowed from older rites. The Cufic, although posterior to Christianity, is probably anterior to Islam for paleographic reasons, as for instance the word ار سلحو, the form حو being found for the affixed pronoun as in the Himyaritic writing. The cave in the flat-topped hill is not of any

great extent, and was probably a cistern. Its name *Sendahanna* suggests the Christian title St Anna.

The caves are certainly not *Troglodyte*, and as Ptolemy in the 2nd century mentions nothing of them there is an additional argument, if such were required, for placing them subsequent to the Christian era. One or two appear to have been inhabited, and are covered with black from the smoke of fires, but here the crosses are as much stained as the rest of the wall, neither more nor less. As the floor shews that it is now used for a sheepfold, the smoke is probably an accumulation of years, this particular cave being for some reason more convenient for the herds.

Mr Paley instanced the Royston cave cut in the chalk, with figures of saints on the sides, but reached by a descending passage, as a somewhat parallel instance.

Mr Lewis stated that the ear of Dionysius furnished a parallel, but the inscriptions there were now illegible.

Mr Bonney asked whether there was any resemblance between these caves and those under the Dome of the Rock, and instanced the Royal caverns at Jerusalem as a case of subterranean quarrying.

Professor Liveing mentioned that the dome-shaped method of quarrying was not very uncommon, as it was often the most economical; and instanced a case in the neighbourhood of Cambridge where the stone had been thus quarried.

Prof. Palmer replied, by briefly sketching out the different kinds of caves in Palestine; he considered that these at Beit Jibrín had no relation with the Dome of the Rock at Jerusalem, for it was quite small. Some caves now used as granaries had probably once been quarries, but many had been excavated for the purpose. He was glad to find that other instances of quarrying in this method could be produced, for he had found that some students of Palestine antiquities had met with difficulties in accepting his views.

On the English sounds of the vowel-letters of the alphabet, on their production by instruments, and on the natural musical sequence of the vowel-sounds. By Mr Potter.

This subject falls under the consideration of the grammarians in their studies of the rules which connect spoken with written language, under the investigations of the physiologists in their discussions of the structure and functions of the organs of the voice and articulation, and under those of the natural philosophers again in their studies of the science of acoustics as the general theory of sound.

The vowel-sounds are shewn by instruments as well as in the voice to be infinitely numerous as they slide or glide gradually from one to another through the whole series or sequence IEAOU from I (i) to U (u), without breaks or discontinuity. Certain sounds of the series are however considered normal sounds, and are supposed to be represented by the vowel-letters of the alphabet; though with little unanimity amongst our grammarians.

The comparison of the speech of different countries is of course a distinct study, and does not fall generally under the present subject of discussion.

The author of the paper having had to lecture through many years on acoustics in the general course of experimental natural philosophy, using, amongst other acoustical apparatus, Kempelen's funnel-shaped instrument and Professor Willis's sliding tubes producing the vowel-sounds, found the sound of the English vowel I (i) not to be given by them, though he was convinced that it was a simple vowel-sound as now used in all large towns, and no diphthong, as many assert it to be from provincial pronunciations.

Where opinions differ so much a reference to actual experiment is the only safe alternative, and a representation of

the organs of the voice and articulation as near as can be made in the ordinary materials at the service of the instrument maker is desirable. The author found the metallic free reeds as they are called, with sheet brass tongues fixed at one end, and vibrating freely in rectangular apertures cut in sheet brass plates, to be the most available substitutes for the chordæ vocales (thyro-arytenoid ligaments) of the human larynx, and india-rubber hollow spheres the best representation of the human mouth, as resonant cavities. The reeds being fitted by short tubes to apertures cut in the hollow spheres to represent the fauces of the posterior part of the mouth, and opposite apertures to represent the opening of the lips being cut, the vowel-sounds are produced by compressing the india-rubber shell when blowing through the reed, to make the shell take the same form as the mouth when producing a like sound.

In this manner the vowel-sounds of I (i) as in the word *pipe*, of E (e) as in *peep*, of A (a) as in *papa*, of O (o) as in *pope* and of U (u) as (oo) in *poop*, are readily obtained, and shew the English sequence of the vowel-sounds to be the most philosophical. The slender (a) as in *paper*, the open (a) as in *father*, and the broad (a) as in *water*, are also easily produced as variations in sound of the first letter of the alphabet, and in English are rightly treated as such.

The sound of I (i) is produced when the back aperture representing the fauces is constricted and that representing the lips left open, and then the vibrating current of expired air is divergent within the mouth, becoming slightly rarefied from the first law of motion by which each particle tends to move in a straight line and with a uniform velocity, and experiment also shews this rarefaction to take place. Such a change in the vibrating stream of air we know by the properties of organ pipes and wind instruments generally affects the tone of the sound escaping to the open air.

On the other hand the sound of U (u) as *oo* in *poop*, is pro-

duced when the front aperture representing the opening of the lips is constricted and the vibrating current of air is convergent within the mouth and slightly condensed, as shewn by experiments; so that another character is given to the sound passing the lips, and the vowel-sound U (u) is produced.

The sound E (e) as in *peep* is produced when the constriction is less than for I (i) and somewhat in front of the back aperture.

The sound of A (a) open as in *papa* is produced when the front and back apertures are both open.

The sound of O (o) as in *pope* is produced when the front aperture and part of the shell near it is somewhat constricted.

The sounds A (a) slender and broad are given by slight compressions of the hinder hemisphere for the first and of the front hemisphere for the latter.

The whole of the vowel-sounds in their infinite variations are thus communicated to a vocal note produced in the larynx by the state of the expired vibrating air as it passes through the mouth to the external air.

Professor MAXWELL thought that these experiments could hardly be connected with those of Helmholz and Donders, as the vowel-sounds differed in different nations.

Mr TROTTER shewed that what might be called pure vowels were very numerous indeed, but that in his opinion *i* was not a pure vowel-sound, and he commented upon Helmholz's investigations in the analysis of sounds.

Mr SHILLETO observed that he thought that *i* in English was always a diphthongal sound.

Mr FENNELL asked if it was known which of the vowels had the greatest and which the least intensity, supposing the fundamental note constant.

Mr POTTER said he thought *a* (as in father) had the greatest intensity, *u* (like *oo* in book) the least, and made some further remarks on the subject of the paper.

Mr H. GOTOBED was elected an Associate of the Society.

May 12, 1873.

The President (Professor Humphry) in the Chair.

On some conditions of reflex action. By Dr M. Foster.

Goltz observed that, while an uninjured frog, placed in a vessel of water the temperature of which was very gradually raised, made efforts to escape as soon as the water became warm, a brainless frog exhibited no movements, and eventually became rigid in the position in which it was first placed. Yet when a brainless frog was so suspended that the toes or feet only dipped into a vessel of water the temperature of which was gradually raised, the feet were always withdrawn by reflex action when the temperature reached 30° C. or thereabouts. The slower the rise in temperature the longer was the withdrawal deferred, but eventually the feet were always withdrawn however gradual the heating of the water. When the whole of both legs was immersed, no withdrawal took place on gradual heating, and the legs became rigid without any attempt to escape having been made. When the legs were immersed up to the knees only, the results were more or less uncertain. It thus appeared that when a sufficiently large surface of the animal was subjected to a gradual heating, reflex actions which otherwise would have taken place were prevented, though the stimulus was by the increase of surface affected largely increased.

The absence of reflex actions in these cases cannot be attributed to diminution of conductivity in the motor or sensor nerves, or of irritability in the muscles, as these are not diminished at the temperatures in question. The author was at first inclined to regard the facts as an example of the more general law of sensation that when a surface of skin is affected

by a stimulus the sensation is most intense at the junction of the affected and unaffected parts (as when the foot is dipped into hot water). But all attempts to get any similar results with other stimuli than heat failed; and an experiment in which the upper part of the body was raised in temperature while the legs were not affected, shewed a great diminution of reflex action in the spinal cord.

Raising the temperature of the spinal cord would naturally be expected to raise (for a time at least) rather than to lower the reflex excitability—but the author has been led by other experiments to conclude that one has to deal here not with simple rise of temperature but with effects of supplying the spinal cord with blood heated above the normal (and therefore possibly carrying in it abnormal products).

The lowering effect of heated blood is shewn by immersing brainless frogs tetanized with strychnia in water at from 30° to 35° C. In a short time all tetanus disappears, and the animal becomes perfectly flaccid, though both muscles and nerves are thoroughly irritable. On removal the tetanus speedily returns, and may be again removed by re-immersion.

The absence of reflex action of the brainless frog immersed in gradually heated water is due to the fact that the gradually heated water is but a comparatively feeble stimulus for the production of reflex action, and before the skin has become sufficiently affected to call forth a reflex action, the spinal cord has become so lowered by the heated blood that it fails to respond by any movement to the stimulus coming from the skin. But inasmuch as a feebler stimulus is needed to awaken consciousness than to produce a mechanical reflex action, the frog possessing a brain begins to move in the heated water at a very early period; and as each movement increases the stimulating effect of the heated water, the movements soon become very general.

The PRESIDENT in proposing a vote of thanks said that the

subject was one of great interest, and that Dr Foster appeared to have clearly shewn that the raising the temperature of the blood had affected the reflex excitability of the spinal chord, and regretted that Dr Foster had not carried his experiments further and applied them to mammals. For example reflex action in human beings in cases of fainting was increased by sudden cold—again, increase of temperature of blood (as in fever) lowered the nerve power—this seemed to correspond with the result obtained by Dr Foster. This depression might result from wear and tear of system, as had often been suggested; but it seemed possible to connect it with the results of Dr Foster's experiments. Again, it might be possible to discover in this way something with reference to the treatment of tetanus, at present so difficult and inscrutable a malady. He thought it would be well to see how far the nerve power in frogs with brains was affected by raising the temperature of blood.

Mr TROTTER enquired if Dr Foster had estimated the amount of heating produced on the spinal chord when one leg only was immersed, and whether that required raising to a higher temperature to produce reflex action.

PROFESSOR MAXWELL mentioned that the effect of cooling certain nerves had been to quicken the circulation.

Dr FOSTER said that these experiments belonged to a different class of facts to those which he had described. With regard to Mr Trotter's question, he had not been able after several experiments to arrive at any very satisfactory results. It was very difficult to get the frog properly placed. He did not think that any very practical result would come with regard to tetanus, for a bath of high enough temperature to affect the spinal cord would probably affect the respiratory functions also. The cause of the lowering of nerve power in fever had yet to be explained.

On the Rete mirabile of the Narwhal. *By* Dr H. S. Wilson.

He divided his remarks into three parts. The first portion consisted simply of the anatomical facts derived from his dissections made of a fœtal Narwhal. The second contained remarks on these dissections as far as they differed from man and from the statements of authors who had investigated the subject. The third part embraced the teleological deductions derived from the facts recorded. In his first portion, after describing the principal sources whence the thoracic rete of the Narwhal derived its constituent vessels, and after pointing out wherein they differed from the arrangement of the same vessels in Man, Dr Wilson proceeded to give a minute account of the position, relations, and structure of the rete itself. He shewed that the rete was divisible into halves, each of which derived its constituent vessels from two sources, that these vessels were peculiar in presenting at their origin the same calibre, being very minute, and consisting of great numbers; that they arose from trunks of the aorta of primary or next to primary calibre, and that, thus, in position, the rete was central to the arterial system. He divided the constituent vessels of the rete into three sets, vasa maxima, v. media, and v. minima, giving their distinguishing characters, and concluded this part with the enumeration of the various structures found imbedded in the substance of the rete. The second portion of his paper had reference chiefly to the discrepancies existing between the notes from his dissections and the statements given by Hunter, Breschet, and Owen, on the thoracic rete of Cetacea in general. In his teleological deductions he attempted to bring the arterial retia mirabilia under headings by dividing them into two great classes, bilateral and axial. The axial he further subdivided into terminal and mediate, and each of these again into complete and incomplete. In commenting on the axial system he

remarked that their probable function was threefold, in some cases to supply a large amount of blood to parts, in others to avoid injury from compression of the vessels, and, in many instances, to check the sudden pressure on nerve centres. In considering the bilateral, after stating that it was found only in Cetacea, he proceeded to give not only Breschet's view of its function as a diverticulum for the storing up of oxygenated blood to be supplied to the circulation during the suspension of respiration, but also the more commonly accepted theory that it is a diverticulum protective against over pressure of blood in the circulation. He inclined to believe that its function embraced both theories. After noticing some deductions derived from the peculiarities in the origin, size, and relations of the constituent vessels of the rete, he concluded with a tabular view of the vessels of the thoracic rete, and of the divisions he had proposed for the arrangement of the various forms of retia mirabilia.

The PRESIDENT, in inviting remarks, enquired what became of the outgoing currents of blood from the rete mirabile, as to how it was distributed.

Dr FOSTER spoke in praise of the paper, and regretted that the specimen was not in a more favourable state for examination; he doubted whether the vasa could be used as reservoirs for oxygen, for the blood at the temperature of the body would speedily oxidate itself with its own oxygen.

Mr TROTTER did not quite see the force of Mr Foster's objection, for this process was the function of the oxygen.

Dr WILSON said the blood returned by the same way as it came. He thought that the storing oxygenated blood was not the *sole* function of the rete; there must, in his opinion, be some other function, for the seal did not present a rete. What this function was could not yet be settled.

New Fellow elected: C. TAYLOR, M.A., St John's College.

May 26, 1873.

The VICE-PRESIDENT (PROFESSOR CAYLEY) in the Chair.

On curves of the fourth degree. By F. W. NEWMAN (communicated by Mr J. STUART)

The curves spoken of in this communication were classified according to their symmetry and the number of their axes.

Prof. CAYLEY said that he considered that the best method of classifying quartic curves was according to the quartic cones of which they were sections. Quartic cones might be divided into singular and non-singular forms; and non-singular forms might be considered to belong to the same species if they could be transformed continuously into one another without passing through a singular form.

(PART XV.)

PROCEEDINGS

OF THE

Cambridge Philosophical Society.

Cambridge:
PRINTED BY C. J. CLAY, M.A.
AT THE UNIVERSITY PRESS.

October 20, 1873.

THE PRESIDENT (PROFESSOR HUMPHRY) in the Chair.

Communications were made to the Society:

(1) *On the Mechanical Means for obtaining the real roots of Algebraical Equations. By* J. C. W. ELLIS.

The general equation

$$A_0x^n + A_1x^{n-1} + \&c. + A_{n-1}x + A^n = 0$$

having been converted (by putting $\cos\theta$ or $\dfrac{1}{\cos\theta}$ for x) into

$$B_n + B_{n-1}\cos\theta + B_{n-2}\cos 2\theta \ldots + B_0 \cos n\theta = 0,$$

various methods were shewn and illustrated by means of models for finding the values of θ.

I. Wheels whose diameters were as $1:2:3$, &c., were connected together by cogs or straps. Long arms were fixed diametrically on their faces, which carried sliding weights. The values of θ corresponding to positions of equilibrium were read off on a dial and gave the roots of the second equation.

II. Another method was by hingeing a number of rods together of variable lengths and causing them to revolve through angles in the ratio of $1:2:3$, &c., round a fixed point in the first rod. This was effected by means of an arrangement of fixed and moveable pullies, when a pencil in the last rod passed through a fixed line determined by the constant in the given equation. The angle revolved through by the first rod was a root of the equation.

In the discussion that followed, Mr GLAISHER, of Trinity College, went briefly into the history of this class of mechanical

inventions, stating that he had met with descriptions of several in the Philosophical Transactions of the Royal Society, and elsewhere. He then put some questions to Mr Ellis respecting the degree of accuracy possible to be attained by such a machine.

Professor MAXWELL described a machine which he had seen at the meeting of the British Association this year at Bradford for shewing the extent and action of the tides.

(2) *Graphic representation by aid of a series of Hyperbolas of some Economic Problems having reference to Monopolies. By* Mr A. MARSHALL.

The price at which a given amount of any commodity can be disposed of in any market is determined by the circumstances of the buyers. If this amount be measured along Ox and this price along Oy, there is thus determined a value of y corresponding to each value of x; and the locus of the points so obtained may be called the demand curve: let its equation be $y = F(x)$. So if y be the price at which an amount x of the commodity can be produced for the market (x, y) is found, the locus of which may be called the supply curve: let its equation be $y = f(x)$. This method of expressing the problem of value has been known certainly for 35 years: an intersection of the two curves has been explained as giving the "average price" about which Adam Smith proved that the "market price" will oscillate. But it has not been pointed out that, under some circumstances, there may be more than one point of intersection, and that Adam Smith's arguments apply only to the circumstances of every alternate point. Only at every alternate point of intersection can the exchange value remain in *stable* equilibrium: at the other points it is in *unstable* equilibrium.

If an individual has the monopoly of the supply of the commodity in the market, his immediate interest will, of course, lead him to determine x so that $x\{F(x)-f(x)\}$ shall be a maximum. Let the curve $y=F(x)-f(x)$ be traced, whether by direct inductions or otherwise, on a paper on which are already lithographed a series of rectangular hyperbolas having Ox and Oy for asymptotes. It will then be obvious by inspection for which of two amounts that the monopolist may throw upon the market—or, which is the same thing, for which of two prices that he may demand—he will obtain the greatest total nett profit. Many striking results can thus be obtained in cases in which the curves cut one another more than once.

This mode of representation of the problem of monopolies is elastic, and lends itself to the treatment of some complex hypotheses. Specially important results will present themselves, if the assumption be introduced that the monopolist is willing to undergo some abatement of his claims, when, by so doing, he can confer great benefit on the consumers.

(3) *A Machine for constructing a series of Rectangular Hyperbolas with the same Asymptotes. By* Mr H. H. CUNYNGHAME.

This machine was intended for the purpose indicated in the last paper.

Annual General Meeting, *October* 27, 1873.

The President (Professor Humphry) in the Chair.

The following officers were elected:

President.

Professor C. C. Babington.

Vice-Presidents.

Professor Liveing.
Professor Maxwell.
Mr Paley.

Treasurer.

Dr Campion.

Secretaries.

Mr J. W. Clark.
Mr Trotter.
Mr J. Batteridge Pearson.

New Members of the Council.

Professor Humphry.
Professor Cayley.
Professor Hughes.
Mr Ellis.
Mr Goodman.
Mr S. S. Lewis.

PROFESSOR HUMPHRY made a communication on certain depressions in the parietal bones of the skull of an Orang and in Man. He showed the skull of an Orang which had been lately presented to the Anatomical Museum by Mr Vores of Caius College, in which these depressions exist. They look as if the bone had been indented on either side of the sagittal suture by the pressure of the finger, the surface being quite smooth and the edges of the depressions bevelled. There was no corresponding alteration in the contour of the interior, the bones being simply thinned at the part. He had not met with a similar abnormity in any other instance of an animal, but had seen it a few times in man, and showed two skulls from the Museum in which it was present, the outer table of the skull being depressed in a considerable area of each parietal bone, and the skull at the parts being quite thin. The remaining bone-structure was healthy, and there was no reason to attribute the condition to disease of any kind or to accident. The appearance and the symmetrical position of the depressions were against both these suppositions. Neither did it seem possible to account for the depressions by any kind of pressure that was likely to occur. Professor Humphry thought they were probably due to a deficiency in the early formative processes in consequence of which the bone had not been produced of proper thickness at these parts, but he could not in the least explain why such deficiency should occur.

A paper was read by C. YULE, B.A., late of St John's, Fellow of Magdalen College, Oxford, "On the Mechanism of opening and closing the Eustachian Tube." In the first part of this paper the arguments in favour of the Eustachian tube being normally closed were reviewed, in consequence of the contrary view having been again revived by Dr Cleland, and some new ones added. The chief point brought forward, however, was the undoubted voluntary power possessed by Mr Yule over his

own Eustachian tube, by which he was able to open and close it at pleasure. When the tube is thus opened, he described the noises made in singing, breathing, &c., as being much intensified, and during loud singing quite unbearable. In order to complete the reasoning logically, the sensations heard in the ordinary ear with a closed Eustachian tube were compared with those heard in the other ear when the tube was kept patent by means of a catheter adapted to the purpose; and in the latter case the modification of hearing was exactly the same as when the tube was voluntarily opened. The second part of the paper was devoted to examining the mechanism of the tube. During the opening of the tube the following points were observed. The soft palate was unchanged in position and form, and hung flaccid; this was important, showing that the tensor and levator palati did not participate in the action; the tongue was not raised, but the only observable change was the approximation of the posterior pillars of the fauces. The reason of this is as follows: The Eustachian tube presents at its inner margin a bluff mass of cartilage which ordinarily occludes the tube; to this is attached the tendon of the salpingo-pharyngeus; below the latter muscle is attached to the palato-pharyngeus, which above arches over to meet its fellow of the other side. At rest the direction of the salpingo-pharyngeus is such as to press the mass of cartilage *into* the tube, but when the palato-pharyngei contract, the insertions of the salpingo-pharyngei are carried inward and a new direction given to these muscles, such that when they contract they tend to draw the lobe of cartilage *out* of the lumen. As the posterior pillars of the pharynx are chiefly made up of the palato-pharyngei, this explains their approximation when an effort is made to open the tube. The clicking sound heard at the commencement of the act of swallowing was pointed out to be due to the separation of the walls of the Eustachian tube.

November 3, 1873.

The PRESIDENT (PROFESSOR BABINGTON) in the Chair.

Mr F. J. CANDY, M.A., Professor of Mathematics, &c. in the University of Bombay, read a paper containing a description of a new Physiological alphabet, devised by himself, to represent the various consonant- and vowel-sounds of the human voice by a series of symbols formed so as to be analogous to the different positions assumed by the mouth, palate, &c. in expressing them. By means of eleven consonant- and three vowel-forms, each admitting of several small modifications, Mr Candy stated his belief that he had included all possible sounds of which any language is susceptible: and illustrated his position by examples taken from the dialects of Hindostan.

The paper was intended as a sequel to one read by him before the Society, on the same subject, May 25, 1857: an analysis of which is given in the "Proceedings" of that date.

No discussion on the merits of the invention followed, it being thought that the system was in too crude a state to be of practical utility.

November 17, 1873.

The PRESIDENT (PROFESSOR BABINGTON) in the Chair.

Mr SEDLEY TAYLOR read a paper "On a suspected forgery in the Vatican manuscript record of the trial of Galileo before the Inquisition."

The preamble of the sentence pronounced in 1633 contains an enumeration of the grounds on which the Inquisition based their verdict of guilty. The existence of unquestionable discrepancies in the face of this document points to a conflict of

evidence only superficially smoothed down—not brought to a definite issue—by the Tribunal. The most serious of these discrepancies relates to an inhibition which the Court asserted had been formally delivered to Galileo by the Commissary of the Holy Office in 1616. From a detailed comparison of Galileo's letters and published works with the contemporary records of the Inquisition, Mr Taylor argued that the documentary evidence on which the judges relied as establishing this point was a fabrication designed to insure the conviction of the accused. He pointed out in conclusion that, if this view be admitted, Galileo must be held entitled, even on the severest view of his legal obligations towards the ecclesiastical authorities, to an absolute acquittal on all the charges.

After a few remarks from Mr GOTOBED, the discussion closed.

Professor SELWYN exhibited a combination of two hoops, united by a straight rod, on which the inner hoop moved, the rod carrying a ball in the centre: the whole being designed to represent the Sun, the orbits of the Earth and Venus, and the conditions under which a transit of Venus between the Earth and the Sun becomes possible.

December 1, 1873.

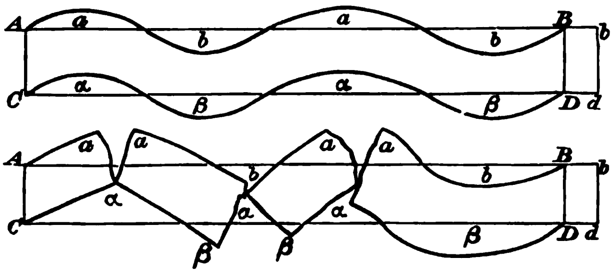
The PRESIDENT (PROFESSOR BABINGTON) in the Chair.

On the Inequalities of the Earth's Surface viewed in connection with the secular cooling. By Mr O. FISHER.

This paper assumes that the elevations and depressions out of which the inequalities of the earth's surface have arisen, are

due to lateral pressure owing to the contraction of the heated interior and consequent wrinkling of the crust to accommodate it to the diminished nucleus.

Let $ABCD$ be a layer of rock of unit of width, length l, and depth k. And suppose the abutments at AC and BD to approach each other through the space le, where e is a small



fraction. Then the layer of rock in question would take some new form, as one of those given in the figure, or any other whatsoever possible.

Call AB "The datum level." Let a, a, &c., be the areas formed by the upper curved line above AB, and b, b, &c., the areas formed by the same line below AB.

In like manner let α, β be similar areas for the lower datum level CD. Then the space included between the curved lines must be equal to

$$Ab\,Cd = kl(1+e).$$

It is also evidently equal to

$$ABCD + a + a + \&c. + \beta + \beta + \&c.$$
$$- b - b - \&c. - \alpha - \alpha - \&c.$$

or, denoting the sums of the quantities similarly situated by the symbol Σ, we get

$$kl(1+e) = kl + \Sigma(a) - \Sigma(b) + \Sigma(\beta) - \Sigma(\alpha).$$

$$\therefore kle = \Sigma(a) - \Sigma(b) + \Sigma(\beta) - \Sigma(\alpha). \qquad (1).$$

Since the pressure is supposed to take place in a horizontal direction, it will not have any direct effect to raise the centre of gravity of the portion of the crust under consideration; so that, if the layer in question rest upon a liquid substratum, we may expect some portions of the disturbed crust to dip into the superheated rocks. But in that case a corresponding volume of such subjacent rock must rise into the anticlinals.

Hence $$\Sigma(\alpha) = \Sigma(\beta).$$

And the equation becomes

$$kle = \Sigma(a) - \Sigma(b).$$

Extending the inquiry to any *area* of the surface of length l and width w, the equation becomes

$$2\,klwe = \Sigma(A) - \Sigma(B),$$

where $\Sigma(A)$ and $\Sigma(B)$ are the volumes of the elevations above, and of the depressions below, the "datum level."

The whole surface of the globe being next taken into account, the relation becomes,

$$\textit{Area of the Globe} \times 2\,ke = \Sigma(A) - \Sigma(B).$$

It is important to understand what is meant by the "datum level." It is an imaginary surface, which occupies the position which the surface of the crust would occupy at the present time, if it had been perfectly compressible, so that no corrugations would have been formed in it by lateral compression. For it would in that case have become simply more dense, without being disturbed in position.

The above relation is applicable to the earth's surface, although that is not strictly regular in its general form, and may contain local elevations and depressions affecting its mean figure,—that is, its mean figure as uninfluenced by lateral compression. For these inequalities, though of small amount as compared with the dimensions of the globe, may be large in

comparison with the quantities of which we have to take cognizance in this investigation. Its truth in no way depends upon the arrangement of the disturbed rocks, nor upon the time at which successive movements have taken place, nor upon the alternate elevations and depressions which have at different times affected any given region. It includes every effect of subsequent denudation, from whatever cause, and to whatever amount. In short, it is perfectly general, so long as it is strictly interpreted. But it does not take account of elevations or depressions of regions of the surface arising from unequal contraction in a radial direction, if their result should be to cause a defect of parallelism between the datum level and the surface of the ocean, to which all our measurements must be in practice referred. However, it does not necessarily follow that contractions in the radial direction will cause depressions in the ocean-bed accompanied with a corresponding increased depth of water. For instance, the defect from a true circular form in the equator affects the surface of the ocean, to which the measurements of geodesy are always referred, so that we do not get an additional mile depth of ocean at the extremity of the shorter radius.

If the earth had cooled as a *solid* body, the outer layers at any epoch having attained their complete amount of contraction sooner than the interior, would have been too large to fit the interior after the cooling had proceeded further. They would therefore have become corrugated. But in this case the corrugation would have necessarily taken place wholly in an upward direction; and there could be no places where any portion of the surface could have become depressed below the datum level. Hence upon this hypothesis we may introduce into our datum-level equation the supposition that $\Sigma(B) = 0$. And it becomes $$\textit{Area of the Globe} \times 2\,ke = \Sigma(A).$$

A little consideration will give the following geometrical relation :

The volume of the Sea above the datum level = the area of the whole surface of the globe × the depth of the datum level below the sea level − the volume of rock displacing water between those levels.

Assuming then that the continents have been shaped out of the master elevations, and that the oceans indicate the positions of the master depressions, and that both are ultimately due to lateral pressure, an estimate of $2ke$ for the whole globe is obtained from the above relation upon the following data:—

(1) The area of the ocean is 146 millions of square miles.

(2) That of the land is 51 millions.

(3) The *mean* depth of the ocean is three miles.

(4) Its deepest parts are about four miles.

(5) The mean height of the land is 900 feet (as shewn by Mr Carrick Moore)[1].

From these data, as a probable value, $2ke = 9504$ feet, which appears more likely to be too small than too large.

The meaning of this in plain language is, that if all the inequalities of the earth's surface were levelled down, they would form a coating 9504 feet thick over the whole globe above the datum level; the datum level being such a surface as has been already defined.

Having thus obtained a value for the thickness of the coating which all the inequalities of the earth's surface would form, if levelled down, a measure of the same thing is sought on physical grounds. For this purpose Sir W. Thomson's paper "On the Secular Cooling of the Earth," is used as a basis to work from[2]. From Mr R. Mallet's late investigations on the contraction of slag from an iron furnace[3], a probable coefficient of contraction for melted rock is deduced, viz. 0·0000217 for 1° Fahr.; and with this is obtained a value for $2ke$, or the thickness

[1] *Nature*, 1872, Vol. v. p. 479.

[2] *Edin. Trans.* 1862; and *Natural Philosophy*, p. 711.

[3] *Royal Soc. Trans.* 1873.

of the coating above defined. Sir W. Thomson's investigation proceeds upon the supposition, founded upon Bischoff's experiments upon the contraction of melted rocks in cooling, that, if the earth, or an outer coating of it, were once in a molten state, then, as soon as a crust began to form, it would break up and sink, and thus the whole would be reduced to the temperature of incipient solidification before it could be permanently crusted over. From the time of such incipient solidification it has gone on cooling, subject to the laws of cooling of a solid.

He then proves that upon this supposition the temperature would increase from the surface downwards, at first at a nearly uniform rate, but at a greater depth much more slowly, until at a certain point such a temperature would be arrived at, as would be about sufficient to induce fusion under the pressure existing at that depth. Now the rate at which the temperature first begins to increase is known to be about 1° Fahr. for 51 feet. Sir W. Thomson has determined, by observation on the rocks at Edinburgh, that their conductivity on an average is 400. With these data he proves that if, for the sake of illustration, the temperature at which the crust began to solidify be taken at 7000° Fahr., then the time since such solidification commenced will have been about one hundred millions of years, and that at about 100 miles below the surface the melting temperature would be reached.

Proceeding upon these assumptions, with the coefficient of contraction for rock above mentioned, the value of $2ke$ is calculated, or the thickness of the coating which all the elevations would form if they were levelled down, and it is found to come out less than 800 feet.

Still further, if instead of 7000° Fahr. 4000° is assumed to be the temperature for melting rock, which seems to be justified by Mr Mallet's experiments, then the value of $2ke$, or the thickness of the coating referred to, would be less than 150 feet. In the latter case the time since solidification commenced would be about thirty-three millions of years.

If we compare the values thus found upon two different suppositions respecting the temperature of melting rock (one of them being extravagantly large) with the value for the same measurement as determined by estimating the actually existing inequalities of the earth's surface, we cannot but be struck with the immense discrepancy between them, the latter being from 12 to 66 times as large as the former. The author is consequently led to doubt the necessity for accepting Sir W. Thomson's restrictions upon the manner in which the earth has come into its present state, especially since it seems now generally admitted that Bischoff's results concerning the contraction of melted rock cannot be relied upon. This was pointed out in 1868 by Mr David Forbes, and quite recently by Mr Mallet, who has determined the contraction in passing from a molten to a solid state to be scarcely 6 *per cent.*, instead of 25 *per cent.*, as stated by Bischoff. Probably, therefore, when we take into account the intermediate condition of viscosity, we need not assume the breaking up and sinking of a crust formed over a molten globe. This view is supported by what Mr Scrope tells us about a lava stream remaining liquid, and even more or less in motion in its central and lower portion for years[1]. Indeed, Sir W. Thomson is careful not to exclude as impossible "the case of a liquid globe gradually solidifying from without inwards, in consequence of heat conducted through the solid crust to a cold external medium."

If this has been what has happened, there may have been a much larger nucleus inclosed within the crust in early times than we have at present, and thus the corrugations formed would have been larger. And a great portion of that nucleus consisting of superheated rocks in a state of igneo-aqueous fusion, much of the water may have escaped in steam during the frequent volcanic outbursts of pristine ages, so that a large portion, at any rate, of the oceans now above the crust may have

[1] *Volcanos*, 2nd ed. p. 84.

been originally confined beneath it; and thus a much greater amount of contraction may have taken place than mere cooling would account for.

It is obvious that this reasoning will apply equally well to the case of a solid globe originally covered with a sufficiently deep layer of molten rock, which is the condition supposed by Sir W. Thomson to be the most probable, a view strongly supported by Dr Sterry Hunt[1], and more in consonance with the rigidity considered requisite to obviate the production of internal tides. But at the same time it is to be remarked, that a highly fluid original condition of the interior may have lasted long after mountains commenced to be formed, and yet its condition need not continue such at the present time.

February 2, 1874.

The PRESIDENT (PROFESSOR BABINGTON) in the Chair.

Mr F. A. PALEY gave a summary of a paper intended to shew that Thucydides must have been mistaken in describing what was really the city-wall of the Platæans, with its battlements and towers, as a temporary wall erected in three months by the besiegers. The paper contended that the Spartan army had got possession of and manned the city-wall, wishing to reduce the Platæans to the necessity of capitulating; and for this a political reason was given. Doubts were thrown on the account of a double wall and double moat, since the researches of modern travellers, which were quoted, did not bear out the statement, and no traces of either existed, though the ruins of the city-walls still remain in great part. It was shewn that ancient Greek cities had precisely such walls as Thucydides describes; and his veracity in the account was impugned, on the supposition

[1] *American Journal of Science*, Vol. v. p. 264.

that he sacrificed strict truth for the purpose of writing a romantic and sensational story.

Mr J. B. PEARSON, of Emmanuel College, read a short paper on Eur. Phœn. 1115—1118, intended to establish its probable genuineness. He pointed out that the legend of Argus was an old and well-known one, and argued that the grammatical difficulties occurring in the passage were not insuperable. Admitting that the poet was desirous to introduce an elaborate and somewhat novel scene out of the legend of Thebes, he suggested that anything uncouth or extravagant in the passage might well be ascribed to poetic licence. Mr Pearson also stated that the authority of the MSS. and Scholiasts was unanimous in recognizing it, as is not always the case with passages intrinsically questionable; and that it was allowed by some, though not all, the best editors, especially Porson, who here dissents from the opinion of Valckenaer whom he generally follows.

February 16, 1874.

The PRESIDENT (PROFESSOR BABINGTON) in the Chair.

(1) *On the geometrical representation of Cauchy's theorems of Root-limitation. By* Professor CAYLEY.

There is contained in Cauchy's Memoir "Calcul des Indices des Fonctions," *Jour. de l'Ec. Polyt.* t. XV. (1837) a fundamental theorem, which, though including a well-known theorem in regard to the imaginary roots of a numerical equation, seems itself to have been almost lost sight of. In the general theorem (say Cauchy's two-curve theorem) we have in a plane two curves $P=0$, $Q=0$, and the real intersections of these two curves, or say the "roots," are divided into two sets according as the Jacobian

$$d_xP \,.\, d_yQ - d_xQ \,.\, d_yP$$

is positive or negative; say these are the Jacobian-positive and the Jacobian-negative roots, and the question is to determine for the roots within a given contour or circuit, the difference of the numbers of the roots belonging to the two sets respectively.

In the particular theorem (say Cauchy's rhizic theorem) P and Q are the real part and the coefficient of i in the imaginary part of a function of $x+iy$ with in general imaginary coefficients (or what is the same thing, we have

$$P+iQ=f(x+iy)+i\phi(x+iy),$$

where f, ϕ are real functions of $x+iy$): the roots of necessity are of the same class: and the question is to determine the number of roots within a given circuit.

In each case the required number is theoretically given by the same rule, viz. considering the fraction $\frac{P}{Q}$ it is the excess of the number of times that the fraction changes from + to − over the number of times that it changes from − to +, as the point (x, y) travels round the circuit, attending only to the changes which take place on a passage through a point for which P is $=0$.

In the case where the circuit is a polygon, and most easily when it is a rectangle, the sides of which are parallel to the two axes respectively, the excess in question can be actually determined by means of an application of Sturm's theorem successively to each side of the polygon, or rectangle.

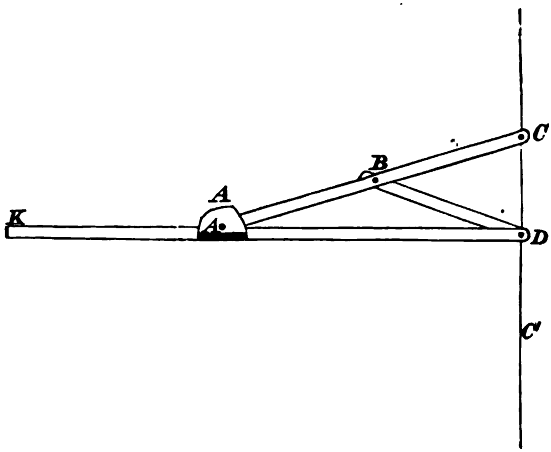
In the present memoir I reproduce the whole theory, presenting it under a completely geometrical form, viz. I establish between the two sets of roots the distinction of *right-* and *left-handed*: and (availing myself of a notion due to Prof. Sylvester) I give a geometrical form to the theoretic rule, making it depend on the "intercalation" of the intersections of the two curves with the circuit: I also complete the Sturmian process in regard to the sides of the rectangle: the memoir

contains further researches in regard to the curves in the case of the particular theorem, or say, as to the rhezic curves $P=0,\ Q=0$.

A communication was also read by Professor Cayley

(2) *On Peaucillier's Parallel Motion.*

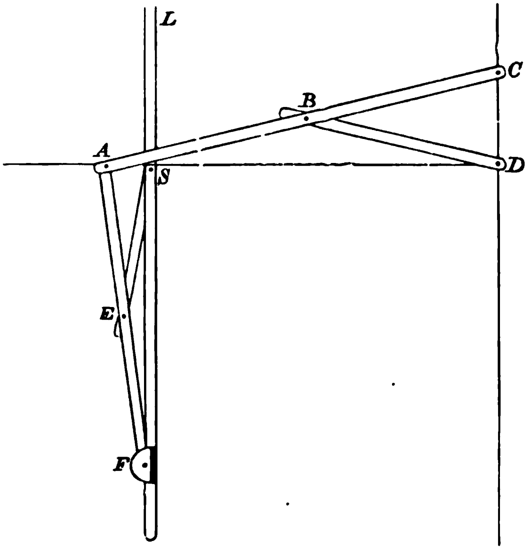
(3) *On some models of Peaucillier's and other Parallel Motions.* *By* Mr Ellis.



DK is a fixed guide upon which A slides. ABC is a rod moveable round a pin at A and bisected in B. BD is a rod whose length is $\frac{1}{2}AC$ and moveable round pins at B and D. It is manifest that C will trace out a straight line CDC' perpendicular to KD. Supposing CAD $(=C'AD)$ when greatest to equal 45°, the space described by C to space described by $A :: 2 : \sqrt{2}-1$, or as 5 : 1 nearly. This method may therefore often be employed with advantage to reduce friction instead of employing a guide for C. The friction may be again reduced almost to any extent by the following arrangement.

The point A instead of sliding is attached by a pin to the

extremity of the rod AF. FSL is a fixed guide at right angles to AD upon which the extremity F of AF slides.



E is the middle point of AF, and ES is a rod equal in length to $\frac{1}{2}AF$, and moveable round pins at ES.

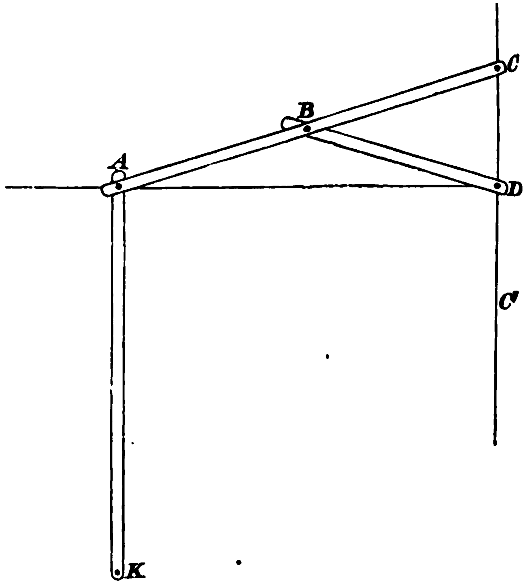
The motion of F may be made as small as we please by increasing the length of AF.

Hence we may approximate as closely as we like to a case of linkwork where the friction is entirely reduced to that round pivots.

For example, if C were attached to a piston-rod whose travel was 10″, the travel of A would be 2″, and the travel of F would be less than $\frac{1}{25}$″, if the rod AF equalled AC.

We might moreover do away with the sliding of F by making it the extremity of a third rod, and so on.

Looking back at fig. I. we see that C would describe a straight line very nearly if KD instead of a straight line were the arc of a very large circle; and this reasoning may originally have suggested to Watt his parallel motion.



Thus if A instead of sliding on a guide be attached by a pin to the extremity of a long link AK, whereof the other extremity K is moveable round a fixed pin k, A will describe a small portion of the arc of a large circle, and therefore move approximately in a straight line. This is in fact a case of Watt's parallel motion. The fixed point k might have been above the line AD.

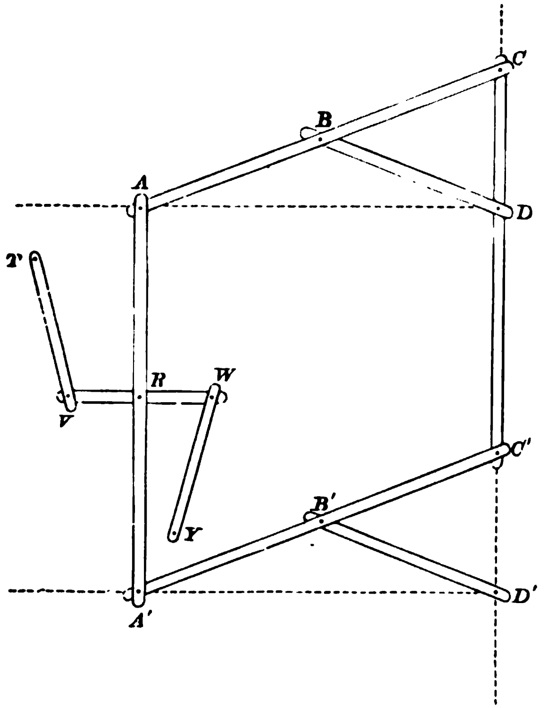
It is to be observed that C will describe an approximate straight line for a considerable space, if A for a short space describes an approximate straight line.

Hence we have only to make A move in a straight line for a short space by any means we can: for instance, by means of Watt's parallel motion.

The above remarks will explain the rationale of the following model in linkwork in the late Prof. Willis' collection.

$ACC'A'$ is a parallelogram hinged at A, C, C', A'; B bisects

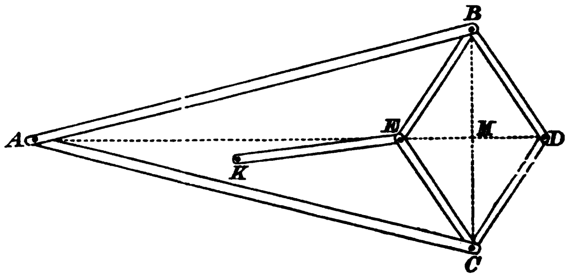
AC and B', $A'C'$; BD, $B'D'$, each equal in length to $\frac{1}{2}AC$, are rods moveable about the fixed pins DD'.



TV, VW, WY are equal rods connected by pins at V and W. VW is attached to AA' by a pin through its middle point R. T and Y are pins fixed in the framework. R therefore by a Watt's motion describes an approximate straight line perpendicular to AA'. Now AA' is parallel to BB', and therefore to DD', which is a fixed straight line. Hence AA' moves parallel to itself.

$\therefore$ A, R, A' describe parts of straight lines perpendicular to AA'.

Hence by what precedes CC' moves in a fixed straight line.



Peaucillier's motion.

BECD is a rhombus formed of equal rods jointed by pins at *B, D, C, E*. It is jointed by pins at *B* and *C* to two equal rods *AB, AC*, which are moveable about a fixed pin at *A*.

It is manifest that the straight line *AE* makes equal angles with *EB* and *EC*, and will therefore if produced pass through *D*.

It is also manifest that

$$AD \times AE = AM^2 - EM^2 = AC^2 - EC^2 = \text{a constant.}$$

Hence whatever curve is traced out by the point *E*, say $\rho = F(\theta)$, the curve traced out by *D* will be $\frac{1}{\rho'} = F(\theta)$, when $\rho = AE$ and $\rho' = AD$. Hence, if *E* is made to describe a circle passing through *A*, *D* will describe a straight line. *E* is made to describe this circle by means of a bridle-rod $KE\,(= KA)$ moveable round a fixed pin at *K*.

March 2, 1874.

THE PRESIDENT (PROFESSOR BABINGTON) in the Chair.

On the Relation of Geometrical Optics to other parts of Mathematics and Physics. By Prof. CLERK MAXWELL.

The study of geometrical optics may be made more interesting to the mathematician by treating the relation between

the object and the image by the methods used in the geometry of homographic figures. The whole theory of images formed by simple or compound instruments when aberration is not considered is thus reduced to simple proportion, and this is found very convenient in the practical work of arranging lenses for an experiment, in order to produce a given effect.

As a preparation for physical optics the same elementary problems may be treated by Hamilton's method of the Characteristic Function. This function expresses, in terms of the coordinates of two points, the time taken by light in travelling from the one to the other, or more accurately the distance through which light would travel in a vacuum during this time, which we may call the *reduced path* of the light between the two points. The relation between this reduced path and the quantity which occurs in Cotes' celebrated but little known theorem, is called by Dr Smith the "apparent distance." The relations between the "apparent distance" and the positions of the foci conjugate to the two points, the principal foci and the principal focal lengths, were explained; and the general form of the characteristic function for a narrow pencil in the plane of xr was shewn to be

$$V = V_0 + \mu_1 r_1 + \mu_2 r_2$$
$$+\frac{1}{2}\frac{\mu_1(r_2-a_2)x_1^2+\mu_2(r_1-a_1)x_2^2-(f_1\mu_2+f_2\mu_1)x_1x_2}{(r_1-a_1)(r_2-a_2)-f_1f_2}+\&c.,$$

where r_1, r_2 are measured from the instrument in opposite directions along the axis of the pencil in the media μ_1, μ_2, respectively, and x_1, x_2 are perpendicular to the axis.

a_1, a_2 are the values of r_1, r_2, for the principal foci, and f_1, f_2, the principal focal lengths, and $f_1\mu_2 = f_2\mu_1$.

If $$\frac{r_1-a_1}{f_1}=\frac{f_2}{r_2-a_2}=\frac{x_1}{x_2},$$

the last term of V assumes the form $\frac{0}{0}$, and an infinite number

of possible paths exist between the points (x_1, r_1), and (x_2, r_2), which are therefore conjugate foci.

Differentiating V with respect to x_1 and x_2 we obtain

$$\frac{d^2V}{dx_1 dx_2} = \frac{1}{D} = -\frac{1}{2}\frac{f_1\mu_2 + f_2\mu_1}{(r_1 - \alpha_1)(r_2 - \alpha_2) - f_1 f_2} + \&c.,$$

D is the quantity in Cotes' Theorems which Dr Smith calls the Apparent Distance, or the distance at which the object must be placed that it may subtend the same angle as when viewed through the instrument.

We have also

$$f_1\mu_2 \frac{dD}{dr_1} = \alpha_2 - r_2, \quad f_2\mu_1 \frac{dD}{dr_2} = \alpha_1 - r_1.$$

March 16, 1874.

The PRESIDENT (PROFESSOR BABINGTON) in the Chair.

Communications were made to the Society:

(1) *On the use of the term Endothelium.* *By* DR MICHAEL FOSTER.

In this paper it was shewn that the term "endothelium" has been recently introduced into histology: and the use of it has rapidly become common if not general. The speedy acceptance of a new term may in many cases, though not all, be taken as an indication that something of the kind was wanted: and the already frequent use of "endothelium," both by Continental and English Histologists, would seem to shew the need of some other phrase besides "epithelium." Nevertheless there are cogent reasons why the new term should not be allowed to take farther root.

In the first place, its etymology is of the most grotesque kind. This is of course an objection of secondary value, but has still some weight. When a term has once come into daily

use with a well-defined meaning attached to it, it does not much matter what its etymology is, or how it is spelt, except on historical grounds. Many terms become so altered in their meanings, before they finally acquire a permanent application, that the chief interest in their etymology is confined to the light it throws on the ideas of the man who first introduced them. This is the chief reason why new terms should be etymologically correct, in order that future inquirers may read back through them into the minds of earlier observers. When a word is etymologically pure nonsense, this is apt to become impossible. Such is the case with "endothelium."

It appears to have been first introduced by HIS to designate the kind of epithelium ("unächte Epithelien") which is found lining the vascular, lymphatic, and serous cavities of the body, in contradistinction to the real epithelium of mucous membranes (see *Die Häute, &c. &c. Akad. Programm.* Basel, 1865). "Sei es, dass man sie als *unächte Epithelien* den *ächten* gegenüber stellt, sei es dass man sie Endothelien nennt, um mit dem Wort ihre Bezeichnung zu den innern Körperflächen auszudrücken."

Endothelium is here contrasted with epithelium, so that the latter may be considered as the "thelium" of free surfaces (whether invaginated or not), and the former as the thelium of internal closed spaces, "thelium" being apparently taken to mean "a layer," or "layers of cells."

Now what is the derivation of "epithelium"? Dr Sharpey gives the following account: he says, in a letter, "epithelium," or rather "epithelida," and especially "epithelia" (1st decl.), was first introduced by F. Ruysch. In describing a preparation of the face of a child finely injected, he refers to the cuticle over the red part of the lip (prolabium), and says, "I cannot call this 'epidermis,' seeing that the subjacent tissue is not skin, but a different substance covered with sensitive papillæ, which are finely injected red." He then goes on to say that as

the cuticle lies on papillæ, he will call it *epithelida* or *epithelia*, from ἐπὶ and θηλή "papillæ," or "mammilla," and he adds that for the same reason he calls the inside coating of the cheeks by the same name. (The original may be found, F. Ruysch, *Thesaurus Anatomicus*, III. No. xxiii. p. 16, "Nulla subest, &c. &c. papillarum"); and again VI. No. cxv. p. 49, he says..."Anterior pars prolabii anterioris—epitheliâ adhuc est obducta...".

From this it is evident that "epithelia" (changed in course of time into epithelium, just as platina becomes platinum) means 'that which covers or is upon a papilla,' and consequently "endothelium" means that which is inside a papilla. The extension of the phrase epithelium to the cellular covering of such parts of the corium as are destitute of papillæ may be easily allowed, but it seems a daring violation of propriety to apply the phrase "within the papilla" to the cells coating surfaces of which one great characteristic is that they are devoid of papillæ! There seems to be something attractive about "thelium" that tempts writers to make use of it. Already "endothelium" has given rise to "ectothelium," and probably "thelium" will soon become a kind of histological maid-of-all-work, with as many prefixes as there are kinds of cells.

In the second place, there are objections to the use of the term endothelium not etymological in their nature. Without considering the peculiar views of His on the connective tissues of the body, it still seems desirable to have some distinctive term to denote the epithelium which is formed out of the elements of the middle of the three layers of the germ (the *mesoblast* of Mr Huxley and myself), the word epithelium being reserved for the nether layer (or *hypoblast*). If so the word endothelium cannot be employed with this meaning, for it would then include structures still called epithelium, and differing in no essential characters from the epithelium derived directly from the hypoblast.

The cells lining the Wolffian duct and its derivative the ureter, with their branches, would then come under the heading endothelium. Whatever be the first formation of the Wolffian duct, whether by the central solution of a solid ridge, or by an infolding of the lining of the pleuro-peritoneal cavity, it is lined by cells which are clearly mesoblastic in origin, not hypoblastic, nor, as was once suggested, epiblastic.

The case of Müller's duct is still more clear. This undoubtedly arises by an infolding of the lining of the pleuro-peritoneal cavity. Its epithelium is distinctly mesoblastic in origin. The germinal epithelium which gives rise to the ovaries is also essentially mesoblastic.

If the word endothelium, then, be taken to denote an epithelium derived from the mesoblast, it must be extended to include the epithelium of the Wolffian and Müllerian ducts, and of the parts which are formed ultimately out of these structures. But if these be included, the phrase loses all its practical utility. If they are excluded, all the little meaning it ever had, vanishes.

It may be urged that we need a word to denote the epithelium which is found in the vascular and lymphatic spaces. There does not however appear to be sufficient reason why the same term should be applied to the whole of this epithelium. As we have seen, its common mesoblastic origin will not justify this. From a structural point of view, three distinct varieties may be recognized in it, viz. the spindle-shaped cells of the blood-vessels and larger lymphatic vessels, the sinuous cells of the commencing lymphatics, and the polygonal cells of the large serous cavities. The fact that the epithelium of the peritoneum is continuous with that of the lymphatics, affords no argument at all for classing them together. We find continuity everywhere. The epidermis is continuous with the alimentary epithelium, and with the urinary and generative epithelium; and the generative epithelium is in turn continuous with the

peritoneal epithelium. In short, there is no reason why the cells spoken of as forming endothelium should have a common title, distinct from the general term epithelium.

The introduction of the new term is really a step backwards from, instead of an advance beyond, the old classification given in Quain's *Elements of Anatomy*, where epithelium is divided either physiologically into epidermic, mucous, glandular, vascular, serous, &c., &c., or structurally, into columnar, spheroidal, ciliated, tesselated, squamous, &c., &c. Some such nomenclature as this satisfies all requirements, either morphological or physiological. The chief morphological importance, as far as our knowledge goes, attaches itself to the question, from which of the three primary layers, epiblast, hypoblast, or mesoblast, any given epithelium is derived; for physiological purposes, all we need is some system of phrases which shall clearly indicate the individual characters and the arrangement of any group of cells; and these requirements are met by the phrases enumerated above. We do perhaps want easy terms denoting whether the epithelium in any spot consists of several layers, or of one pronounced layer only; *monoderic* may be proposed for the latter, *polyderic* for the former case. Epithelium itself would only mean cells lining a cavity or coating a free surface.

(2) *On some Problems on the Physiology of Nutrition, and the methods of solving them. By* Dr MICHAEL FOSTER.

(3) *On an Experiment of Galileo. By* Mr SEDLEY TAYLOR.

Mr Sedley Taylor drew attention to an observation made by Galileo, and described by him in the first of his *Dialoghi delle nuove scienze*[1]. Galileo says that while scraping a brass

[1] Vol. XIII. pp. 104, 105, of the Florentine edition of Galileo's complete works.

plate with a chisel in order to remove some spots, he noticed that the passage of the instrument across the plate occasionally produced a powerful and distinct musical note, and that, when this happened, a long row of fine equidistant striations was left on its surface. These marks were closer together when the sound was acute than when it was grave. Having produced by the above means two notes which differed by an exact Fifth, Galileo measured the distance between their respective striæ, and found that *three* of those corresponding to the upper note occupied precisely the same space as *two* corresponding to the lower. He hence inferred that the numbers of vibrations executed in the same time by any two notes forming this interval are in the ratio of 3 : 2; a conclusion which had previously been only conjectured from results obtained by the monochord. Galileo remarks further that the same principle applies to the case of *any* interval.

Mr Taylor exhibited a brass plate with rows of striæ upon it obtained by screwing the plate into a lathe, and, while it was rotating, holding the edge of a chisel against it in such a way as to produce a musical sound. The markings were in some cases extremely fine and regular.

April 27, 1874.

The PRESIDENT (PROFESSOR BABINGTON) in the Chair.

Communications were made to the Society:

(1) *On the use of the "Ligamentum Teres" of the hip-joint. By* Mr SAVORY, F.R.S.

This paper discussed the proper use of the "ligamentum teres," which, though variously stated, has not, it was maintained, been correctly given. The statement that the ligament is vertical and tight, when the person is erect, has been chal-

lenged: but the author was satisfied of its accuracy. It could be demonstrated by removing the bottom of the acetabulum with the trephine. The ligament is moderately tight when a person stands evenly upon both legs. It is tighter when the femur is slightly flexed, as it usually is. But when resting upon one leg, inasmuch as the pelvis is then raised on that side, which of course affects the ligament in the same way as adduction of the femur would do, then the ligament becomes extremely tense. In other words, it becomes tightest when the hip-joint has to sustain the greatest weight. When therefore the pelvis is borne down upon the femur, or when the femur is forced upwards—that is when the pressure would be greatest between the upper part of the acetabulum and the opposite surface of the head of the femur—it is put directly on the stretch. More precisely, its great purpose is to prevent undue pressure between the upper portion of the acetabulum, just within the margin, and the corresponding part of the head of the femur. But for this ligament such undue pressure must inevitably occur. Suppose the *ligamentum teres* absent and the person standing upright, owing to the obliquity of the acetabulum and the head of the femur, pressure between the two could not be equally, or nearly equally, diffused over their opposing surfaces, but it would be concentrated on a spot in the upper part of the socket through which a line drawn down the body, through the joint into the leg, would pass. When the thigh is straight, when the femur is in a line with the body, as when one stands upright, then is the ligamentum teres in the same line too, and consequently any force which drives the femur and pelvis together must tell at once upon the ligament, and be directly checked by it. Owing therefore to the shape and obliquity of the hip-joint, and the weight of the body, the ligamentum teres is necessary to prevent concentration of pressure at a particular point above it. The line through which the weight or force acts between the upper

part of the acetabulum and the opposed surface of the head of the femur, forms, with the line of weight of force which passes through the ligamentum teres, an obtuse angle: and the resultant of these forces is in a line which passes through the long axis of the head of the femur. When the person is erect, the body partly hangs upon the ligamentum teres. This, he submitted, is the prime function of the ligamentum teres. Other purposes he did not deny, but would maintain that they only occasionally come into play, and are altogether subordinate to this one, which is especially called into action whenever the weight of the body is thrown upon one leg. He supported his view by reference to comparative anatomy, remarking that it is present when the acetabulum looks outward, and the head of the femur is inclined inward; in other words, when the hip-joint is placed obliquely, so that there would be otherwise undue pressure at a particular part; and that it is absent in those animals in whom, although it is an instrument of regression, the posterior extremity does but little in supporting the weight of the body; e.g. seals, and the ourang-outang.

In a discussion which followed, Prof. Humphry disputed, and Mr Savory still maintained, the tension of the ligament referred to in the paper.

(2) *On a Clepsydra. By* Mr Ellis.

(3) *A Model shewing the mechanical arrangement of the Joints in the Limb of a Lobster. By* Mr Ellis.

May 11, 1874.

THE PRESIDENT (PROFESSOR BABINGTON) in the Chair.

The following communication was made to the Society:

On the Bearing of the Distribution of the Portio Dura *upon the Morphology of the Skull.* *By* T. H. HUXLEY, *Sec.* R.S.

In the first place, the distribution of the seventh nerve or *portio dura* in Man was compared with that of the same nerve in the *amphibia;* and it was shewn that, while the proper facial nerve, with the *chorda tympani*, corresponds in all essential respects with the posterior division of the seventh nerve in the Frog and other amphibia, the *nervus petrosus superficialis major* or vidian nerve, with its palatine branches and the nerve of Cotunnius, answers to the anterior division of the seventh, or so-called "palatine" nerve of the Frog. A branch which, in the *Urodela*, connects the *portio dura* with the Gasserian ganglion, appears to be the homologue of the *nervus petrosus superficialis minor.* The tympano-Eustachian passage, in both Man and the Frog, is included between the two main divisions of the *portio dura.*—The distribution of the seventh nerve in the Ray was next described. Its two divisions were shewn to have the same relation to the spiracle as they have to the tympano-Eustachian passage in the higher vertebrata. The anterior division, however, differed from that of the Frog and that of Man, in possessing no branch comparable to the nerve of Cotunnius. The place of this nerve appears to be taken by a large 'palato-nasal' branch of the fifth (as Bonadorff has already suggested), and it was suggested that the Cotunnian branches of the palatine nerves in the Frog and in Man really belong to the Trigeminal. The distribution of the *portio dura* was then

compared with that of the glossopharyngeal and that of the branchial branches of the vagus, and the conclusion was drawn, that the *portio dura* is the nerve of the mandibulo-hyoid cleft (commonly called the first visceral cleft), and is distributed to the (morphologically) anterior and posterior walls of that cleft. As a corollary from this conclusion, it followed that the pterygoid arcade does not represent an independent visceral arch, but is a dependence of the mandibular arch, as Gegenbaur has already argued upon other grounds. It was further shewn that the distribution of the second and third divisions of the fifth nerve is such as accords with the view that they represent the posterior division of the nerve of the trabeculo-mandibular cleft. The anterior division of that nerve was sought in the palato-nasal branch of the trigeminal—while the first division of the latter nerve appears to be the nerve of the (morphologically) anterior face of the trabecula. The sixth, third, and fourth nerves were regarded as special branches of the nerves of the mandibulo-hyoid, and trabeculo-mandibular clefts respectively, developed in relation with the special muscles of the eye. The author finally endeavoured to shew that the results thus obtained by the thorough investigation and comparison of the distribution of the cranial nerves were in entire accordance with those obtained by the study of development, and that the apparent anomalies in the distribution of the fifth and of the seventh nerves in the higher *vertebrata* are easily explained by the metamorphoses of the trabecular and mandibular and hyoidean arches in these animals.

Professor HUMPHRY expressed his thanks and the thanks of those present to Prof. Huxley for the careful and lucid account which he had given of a difficult piece of anatomy, and for the interesting and morphological inferences which he had deduced from them, and also for the illustration he had given of the fact that the dullest, most troublesome anatomical details may be brightened, and so rendered easy by an insight into their true

meaning. This was really the way to study anatomy, viz. to regard the various facts in connection with other facts, and so as the bases of scientific deductions. Prof. Humphry was glad to hear the nerves thus made the exponent of cranial morphology, for he had attempted the same thing many years ago in a paper read at the British Association at Leeds, when he endeavoured to shew that the fore limb was not, as supposed by Prof. Owen, an appendage to the skull, but formed independently from it. He then shewed, from a consideration of the distribution of the cranial nerves, that the hyoid and not the scapula is the visceral arch of the occipital, and that the mandibular, the pterygo-maxillary and the ethmo-vomerine arches are the respective visceral arches of the post-sphenoidal, the pre-sphenoidal, and the ethmoidal parts of the skull. This view he believed to be in the main correct. The nerves respectively supplied to them are the ninth and the three divisions of the fifth. Each of the latter is very closely confined to its particular visceral arch, sending a special nerve to each bone of its arch, or nearly so, whereas the seventh pair of nerves is more promiscuous in its distribution, being supplied to muscles disposed upon all the four visceral arches, and having connecting links with the spinal nerves of those arches. It was to the orderly disposition of these connective links in relation to the visceral arches that Professor Huxley had now called their attention. Professor Humphry remarked that the communication between different nerves, which is a means of establishing the harmonious action of the several muscles supplied by them, was effected in three ways. *First*, by junction of their terminal branches. This is most common in the lower animals. *Secondly*, by plexuses near their origin from the brain and spinal cord, which are found, to some extent, in the lower animals, but which are more numerous in the higher animals. *Thirdly*, by means of ganglia. This last, which may be regarded as the most perfect method, is almost confined to the higher animals. Accordingly the communicating

branches between the seventh and fifth, which formed the subject of the author's paper, pass to Meckel's ganglion, the otic ganglion, and the submaxillary ganglion in Mammals; whereas in Batrachians they do not pass to these ganglions, but their junctions are effected among the terminal ramifications of the nerves. He could not agree with Prof. Huxley that the fore part of the skull was not, like the hinder part, composed of vertebral elements. It was transversely segmented after the manner of the rest of the skeleton, and these segments are vertebræ, whether the notochord exists at the part or not; and whether the segmentation takes place early or not, that is, in the cartilage or in the osseous nuclei developed in the cartilage, makes little difference. Sooner or later, in the higher animals at any rate, the segmentation occurs. The foremost elements derived from the trabeculæ had been designated as ribs by Prof. Huxley: and if they are so, they are components of those segments, of which the vertebræ form the mesial elements. He could not quite accept the view of the homologies of the mandibular arch which had been given; but time failed to discuss these questions more fully. He concluded by again thanking Professor Huxley for this interesting communication.

May 25, 1874.

The President (Professor Babington) in the Chair.

Mr Pearson read a paper on some meridian observations of the Sun taken by him with a prism-circle and an artificial horizon, at Taormina in Sicily, on April 1st last. They were taken with the view of determining the latitude of the place. The watch was set to Greenwich time, but about 8m. 9s. slow.

The observations were taken before and after noon, and, reduced, were as follows:

	Time. h.	m.	s.	Altitude.		
(1)	10	46	0	56°	41'	29''.
(2)	10	49	30	56°	43'	29''.
(3)	10	53	0	56°	44'	59''.
(4)	10	58	50	56°	44'	19''.
(5)	11	1	0	56°	43'	9''.
(6)	11	3	40	56°	41'	29''.
(7)	11	12	40	56°	30'	19''.

A comparative examination of these suggested 10h. 54m. 35s. as the probable time of apparent noon. (The method for ascertaining the time of noon given by Godfray, Ast. art. 150, was not available at the time the observations were worked out: by it, the times of app. noon on the mean of two separate observations are as follows: (1) and (2) 10h. 54m. 0s., (3) and (4) 10h. 54m. 40s., (4) and (5) 10h. 54m. 25s., (5) and (6) 10h. 54m. 15s....average 10h. 54m. 20s.)

The method employed to find the latitude is that given in Raper's "Navigation." Tables are given containing a given series of numbers varying for all latitudes and declinations. The number in this particular case (Lat. 37°. 50'. 50'' N., Sun's Decl. 4°. 34'. 6'' N.) is ·458.

This is added to the sin. sq. of the time elapsing between the time of observation and that of apparent noon: the result is the log. of the sin. of the difference between the altitude of the sun at the time of observation and its meridian altitude.

Employing this method we get these results: For observation (1) Lat. 37°. 49'. 0''; for (2) 49'. 19''; (3) 48'. 56''; (4) 49'. 0''; (5) 49'. 0''; (6) 48'. 53''; (7) 48'. 43''; average 37°. 48'. 58''. 7.

The methods given in Norie's "Navigation," and in Godfray's Ast., art. 149, produce very nearly the same results; e.g. obs.

(6) worked out (a) on Norie's method gives Lat. 37°. 49'. 10'',
(b) on Godfray's method gives Lat. 37°. 48'. 54''.

The hour-angle of the Sun, by obs. (7), on the theory (1) that the lat. of the chart is correct, and that the instruments were in adjustment, is 16m. 45s.—an error of 1m. 20s.; (2) that there was an error of about 2' in one of the two, is 17m. 57s.—an error of 8s., which tends to prove the existence of some such error.

The present Admiralty chart, issued as newly corrected, 1873, gives 37°. 50'. 50'' as the lat. of the spot where the observations were taken. At first sight, this would seem to prove an error of nearly 2', either in the instrument or the artificial horizon, as levelled at the time.

But (1) Admiral Smyth, in his survey of Sicily, carried out in 1813—15, places Taormina in 37°. 48'. 40''. (2) He states in his book on the subject that he was remarkably well supplied with sextants and other surveying instruments. (3) The long. and lat. of the principal points on the coast given by him often agree with those now given in the charts. (4) His estimate of the height of Etna, obtained by triangulation from a base on the sea, viz. 10,874 ft., is *very* nearly accurate, that recently obtained by levelling being 10,840 ft. (5) The lat. of Taormina, as now given in the charts, agrees exactly with, and may possibly be borrowed from, that given by Baron von Waltershausen, in his survey of Mt. Etna and its environs, executed from 1840 to 1850, and may therefore be not perfectly accurate.

On these grounds it was argued that the latitude of Taormina, as given by Adm. Smyth, and (approximately) by this set of observations, may perhaps be more nearly accurate than that given in the present charts: at any rate they shew that it is perfectly feasible for a person, with simple instruments and merely arithmetical processes, to determine his latitude, in any part of the globe, with reasonable accuracy.

Dr CAMPION said, that from practical experience, he was

aware of the uncertainty attending any set of observations made by a single individual.

Prof. CAYLEY suggested a diagram, similar to those given in meteorological reports, indicating, by a curved line and dots, the altitude of the Sun at different times: from which the meridian altitude and its time might be approximately inferred.

The Secretary then read a paper

On the Temperature of the Earth in times anterior to the Eocene period. By Mr RÖHRS.

Mr RÖHRS stated that geological evidence seemed to point to a warm and equable climate over a great part of the earth in præeocene days. He thought it probable that this high temperature was due to the internal heat of the earth, and that the amount of heat radiated by the sun and received by the earth may have been less than it is now—the solar atmosphere obstructing radiation more than at present, although the energy and mean temperature of the sun were greater in early times. He referred the first great glacial period to a time when the internal heat of the earth was diminishing, and the solar radiation had not reached its present amount.

Mr SOLLAS said that there was clear evidence of ice-action at various epochs long anterior to the Eocene period, so that Mr Röhrs' theory of a long period of uninterrupted high temperature was geologically untenable.

October 19, 1874.

Professor Cayley in the Chair.

On some Ice-hummocks in the Gorner Glacier. *By* Mr Trotter.

The speaker described some remarkable water-holes associated with hummocks of ice on parts of the Gorner Glacier which he had observed in 1863 and in 1874. The water-holes were oval in shape with their longer axes parallel and pointing east and west, the hummocks were on the south side of the water-holes. The direction was independent of that of the veined structure, and the whole was obviously a meridian phenomenon, but several points as to the origin of the hummocks and the shape of the water-holes were very obscure.

An examination of them earlier in the season would probably throw some light on their origin.

ANNUAL GENERAL MEETING, *October* 26, 1874.

The PRESIDENT (PROFESSOR BABINGTON) in the Chair.

The following were elected officers for the ensuing year:

President.

Professor CHARLES C. BABINGTON, F.R.S.

Vice-Presidents.

Professor MAXWELL.
Professor MILLER.
Mr MUNRO.

Treasurer.

Dr CAMPION.

Secretaries.

Mr J. W. CLARK.
Mr TROTTER.
Mr PEARSON.

New Members of Council.

Professor LIVEING.
Mr JACKSON.
Mr GLAISHER.

On a nearly complete Skeleton of the Bos Primigenius found in Burwell Fen. By Mr J. W. Clark.

Mr J. W. Clark exhibited and made some remarks upon a skeleton of the great extinct Ox (*Bos primigenius*). The bones had been found together in Burwell Fen early in the spring of 1874; and there could be no doubt that they belonged to the same animal. The parts wanting are the right hind-leg, one lumbar vertebra, a few terminal vertebræ of the tail, and a few bones of the carpus, tarsus and toes. The skeleton, after the bones had been properly treated with gelatine, had been mounted and placed in the Museum of Comparative Anatomy. It is the first skeleton found in England in a sufficiently perfect state to allow of its being articulated. Mr Clark briefly recapitulated the history of the species, shewing, from the passages out of chronicles and other contemporary records collected by Mr Boyd Dawkins, that it had subsisted in a living state on the continent of Europe down to a much later date than had been supposed previous to his researches.

November 2, 1874.

The President (Professor Babington) in the Chair.

On some further Observations with a Prism-circle. By Mr Pearson.

This paper was intended as a sequel to one read on May 25th, the two being intended to establish by practical examples the facility with which a traveller may establish his latitude and longitude in any part of the globe.

The following observations were taken Sept. 5th a.m. in Lat. 52° 12' 10" N.; Long. 30s. (7' 30") E. The time given is local mean time.

h. m. s.	Sun's Alt.	h. m. s.	App. dist. of Sun from Moon.
7 42 27	21° 25' 40"	7 47 36	62° 54' 40"
7 53 44	23° 6' 0"	7 51 1	62° 53' 40"

The observation of the Sun at 7h. 42m. 27s. giving the local time obtained from the Philosophical Society's clock accurately within 7s., the levelling of the art. horizon, and so the altitude of the sun may be taken as nearly correct.

The above observations reduced give

At 7h. 48m. 19s. (L. M. T.) alt. of Sun's centre 21° 59' 42".

True app. dist. of centres of Sun and Moon 63° 25' 54".

The Moon being in 28° N. Dec. and having just passed the meridian (at 7h. 44m. 11s.), the altitude was obtained by calculating the Reduction to the meridian, instead of by the more difficult method of observation, the change in alt. in 4m. 8s. being only about 44". This gave the true altitude of the Moon's centre 65° 47' 50". Of course the altitude of the Moon could only have been approximately ascertained in this way, had not the longitude of the place of observation been accurately known.

These data were worked out on four methods: 1st, on the plan given in Woodhouse's *Astronomy*, which is believed to be based more or less nearly on the formula originally devised by the Chevalier Borda; 2nd, on that given in the Introduction to Shortrede's Logarithmic Tables, mainly identical with the first method; 3rd and 4th, on the two methods given in Arts. 700, 701 of Raper's *Navigation*.

No. 1 gives the longitude of the place of observation 1s. W. of Greenwich; error 31s.

No. 2 gives it 3s. E. of Greenwich; error 27s.

No. 3 makes it 12s. E. of Greenwich; error 18s.

No. 4, 3s. W. of Greenwich; error 33s.

A second set of observations gave at 8h. 5m. 13s. local time, true alt. of Moon 65° 29' 20"; true alt. of Sun 24° 10' 0"; true app. dist. of their centres (mean of three observations) 63° 20' 27".

These data, when computed on the 1st method mentioned above, give the long. of the place 1m. 35s. E.; error 1m. 5s.: on the 4th method 1m. 29s. E.; error 59s. It was thought unnecessary to make the calculations again on the 2nd method, because its form is nearly the same as that of the first; on the 3rd, because its result in the previous case differed considerably from the other three.

The bar. and ther. were not taken into account, as neither of them were far from the point at which they do not affect the refraction. The index error of the instrument was too small to be ascertained by any one but a very good observer. The inaccuracy of the second set of observations might be due to a haze coming on at the time, and to the increasing brightness of the Sun.

Several extracts were also given from the voyages of Cook and Krusenstern, bearing on the accuracy with which it was found practicable to use this method of fixing the long. at the end of the last and the beginning of the present century. For example, the long. of Santa Cruz (Teneriffe), where Cook is described (Voyage, Vol. I.) as having met Borda in August, 1776, was given by the latter as 18° 35' 30" W. of Paris, Cook making it by his timepiece 16° 31' 0", by two sets of lunars 16° 30' 45" W. of Greenwich. The true long. as given by the English Admiralty Chart (1873) being 16° 14' 56" W. of Greenwich, or 18° 35' 6" W. of Paris.

Nov. 16, 1874.

THE PRESIDENT (PROFESSOR BABINGTON) in the Chair.

Linear partial Differential Equations, and their Germ-integrals. *By* S. EARNSHAW.

This paper will be found printed at length in the "Transactions of the Society."

[*Abstract.*]

Long before it was discovered that

$$u = \frac{A}{\sqrt{x}} \cdot \epsilon^{2\sqrt{xy}} + \frac{B}{\sqrt{x}} \cdot \epsilon^{-2\sqrt{xy}}$$

is an integral of the equation $\frac{d^2u}{dxdy} = u$, it had been known that every linear partial differential equation with constant coefficients, whatever be the number of its variables, is susceptible of an integral of the form

$$u = C\epsilon^{mx+ny+\dots}.$$

It thus appeared that the above equation admits of two integrals of essentially different types. The same was found to be the case with the equation $\frac{d^2u}{dx^2} = \frac{du}{dy}$; of which both

$$u = C\epsilon^{mx+m^2y}$$

and

$$u = \frac{A}{\sqrt{y}} \cdot \epsilon^{-\frac{x^2}{4y}}$$

are found to be integrals; and they are also of essentially different types as integrals. This discrepancy of types created in me a desire to ascertain the significance and true origin of each, and their mutual dependence if any existed.

I have worked out the case of two independent variables, but the method I have adopted is applicable to an equation of any number of independents. It is shewn that in every integral certain constants, called *germs*, exist, or can be arbitrarily introduced into it if they do not already exist there; and by means of these a *germ-integral* can be found; and from this a series of *sub-integrals*; and the sum of these is the general integral of the proposed equation.

This method depends for its success on the circumstance that the differential equation from which the sub-integrals are obtained contains fewer independent variables than the proposed equation. Hence when the equation to be integrated is the general differential equation of the second order of two independent variables and constant coefficients, the sub-integrals are to be found from a differential equation of the second order of one independent variable; and by its integration that of the general equation of the second order is accomplished.

The same method is shewn to be successful in the integration of certain other equations where the coefficients are not constants, but functions of the independent variables.

Nov. 30, 1874.

PROFESSOR HUMPHRY in the Chair.

(1) *On Lopsided Generation, or Right-handedness. By* W. AINSLIE HOLLIS, M.D. Cantab. This paper was read by Professor Humphry in the absence of Dr Hollis.

The antiquity and universality of the preferential use of the right hand was shewn by reference to the Biblical and other

records, and to Egyptian, Assyrian and other monuments, as well as to various members of the Semitic and Aryan groups of languages. All modern nations, with one or two questionable exceptions, are right-handed, and have words to signify "left-handed" corresponding with the French "gauche" and the Italian "mancino." It appears to be a peculiarity of the human race, even the Apes using the right and left limbs indiscriminately, and is associated with the higher and more elaborate muscular actions of the limbs in Man; and there being no other structural difference between Man and the lower animals to account for it, the cause of the peculiarity must be sought in that part of the system, viz. the brain, in which he excels other animals. The left side of the brain was stated to be the larger in Man; and it, through decussation of the nerve-fibres, presides over the right side of the body, and seems from recent observations also to preside over the complex and delicate muscular actions upon which articulation depends. The preponderance of the left side of the brain—the lopsidedness of the organ—thus engendered by the preferential use of the right hand, by the movements in speech and by much of subsidiary brain-work directly associated with speech, is not without its evil; and instances were adduced, including those of Johnson and Swift, in which the left side of the brain had suffered and paralysis of the right side of the body had been induced, apparently, as a consequence of this overwork. The inference was drawn that such result might have been avoided had a more equal duty been required of the two sides of the brain by a more equal use of the two limbs; and in these days of high pressure it is of especial importance to attend to such points, and by more equal education of the two sides of the body, to lead to a fairer distribution of work between the cerebral hemispheres.

Professor Paget thought that more evidence should be adduced respecting the greater size of the left hemisphere of

the brain, and as to which side of the brain is likely to be affected when Aphasia occurs in left-handed persons.

Mr ANNINGSON questioned whether lopsidedness was really a part of right-handedness, forasmuch as the left hand is employed not only as a helpmate to the right, but for many purposes in which the right hand is less efficacious.

Mr CARVER thought the observations in the nursery shewed that right-handedness was acquired rather than innate; children having commonly a propensity to use the left hand, which it required some difficulty to counteract.

Professor HUMPHRY stated that the paper, which was one of much learning and interest, as well as suggestive, had been consigned to him for publication in the next number of the *Journal of Anatomy and Physiology.* In reply to various questions which had been asked, he said he believed an advantage gained by preferential use of the right hand, was a greater aptness and precision of movement requisite for delicate manipulations than could have been attained had both limbs been equally employed. Left-handed persons, being prevented by social custom from concentrating their attention on the left hand and being compelled to give a frequent preference to the right, are at some disadvantage in this respect. He could see no anatomical reason for the preference of the right limb, the slight advantage in circulation to the right arm through the innominate artery and vein applying, in nearly equal degree, to the right side of the brain. He agreed with Mr Carver that right-handedness was much a matter of education, and followed from the multifarious single-handed offices which are associated with the higher mental endowments.

(2) *On the Peritoneum in Man and other Vertebrates.*

Dr WILSON made a communication on the disposition of the peritoneum in Man and other vertebrata. He gave a brief account from his own dissections, of the anatomy of the peritoneum, and more particularly of its omental sac in Man and many Mammals, Reptiles, Amphibians and Fishes. He shewed that in many of these the omental sac is divided into two parts—a gastro-hepatic and a gastro-colic part—by a constriction corresponding with the upper border of the stomach. This he first observed in the dissection of a Narwhal, and had found it marked to a variable extent in Man, most evident in a young Hippopotamus, distinct in the Rat and in the human fœtus about the 3rd month. In Reptiles and Amphibians the omentum does not extend below the level of the stomach. There is therefore only a more or less complete representative of the gastro-hepatic part of the omental pouch of Man. One or more of the hepatic lobes usually project into the gastro-hepatic part of the sac. In Man it is the lobulus Spigelei. He described the relation of the spleen to the omental pouch, and stated that his observations were, on the whole, in accordance with the old and commonly received view regarding the mode in which the colon is embraced by the two recurrent layers of the omentum which pass on to form the transverse meso-colon.

Professor HUMPHRY remarked on the thorough manner in which Dr Wilson had investigated the anatomy of the omentum, which was of much interest with reference to the development of parts. The increasing size of the omental pouch in the higher animals and in Man must also be taken in connection with the recent investigations of Dr Klein respecting the relations of the peritoneal cavity to the lymphatic system.

Feb. 8, 1875.

The PRESIDENT (PROFESSOR BABINGTON) in the Chair.

On the Centre of Motion of the Eye. *By* PROF. CLERK MAXWELL.

The series of positions which the eye assumes as it is rolled horizontally have been investigated by Donders (Donders and Doijer, *Derde Jaarlijksch Verslag betr. het Nederlandsch Gasthuis voor Ooglijders.* Utrecht, 1862), and recently by Mr J. L. Tupper (*Proc. R. S.*, June 18, 1874). The chief difficulty in the investigation consists in fixing the head while the eyeball moves. The only satisfactory method of obtaining a system of co-ordinates fixed with reference to the skull is that adopted by Helmholtz (*Handbuch der Physiologischen Optik*, p. 517), and described in his Croonian Lecture.

A piece of wood, part of the upper surface of which is covered with warm sealingwax, is placed between the teeth and bitten hard till the sealingwax sets and forms a cast of the upper teeth. By inserting the teeth into their proper holes in the sealingwax the piece of wood may at any time be placed in a determinate position relatively to the skull.

By this device of Helmholtz the patient is relieved from the pressure of screws and clamps applied to the skin of his head, and he becomes free to move his head as he likes, provided he keeps the piece of wood between his teeth.

If we can now adjust another piece of wood so that it shall always have a determinate position with respect to the eyeball, we may study the motion of the one piece of wood with respect to the other as the eye moves about.

For this purpose a small mirror is fixed to a board, and a dot is marked on the mirror. If the eye, looking straight at the image of its own pupil in the mirror, sees the dot in the

centre of the pupil, the normal to the mirror through the dot is the visual axis of the eye—a determinate line.

A right-angled prism is fixed to the board near the eye in such a position that the eye sees the image of its own cornea in profile by reflexion, first at the prism, and then at the mirror. A vertical line is drawn with black sealingwax on the surface of the prism next the eye, and the board is moved towards or from the eye till this line appears as a tangent to the front of the cornea, while the dot still is seen to cover the centre of the image of the pupil. The only way in which the position of the board can now vary with respect to the eye is by turning round the line of vision as an axis, and this is prevented by the board being laid on a horizontal platform carried by the teeth.

If now the eye is brought into two different positions and the board moved on the platform, so as to be always in the same position relative to the eye, we have to find the centre about which the board might have turned so as to get from one position to the other.

For this purpose two holes are made in the platform, and a needle thrust through the holes is made to prick a card fastened to the upper board. We thus obtain two pairs of points, AB for the first position, and ab for the second.

The ordinary rule for determining the centre of motion is to draw lines bisecting Aa and Bb at right angles. The intersection of these is the centre of motion. This construction fails when the centre of motion is in or near the line AB, for then the two lines coincide. In this case we may produce AB and ab till they meet, and draw a line bisecting the angle externally. This line will pass through the centre of motion as well as the other two, and when they coincide it intersects them at right angles.

February 22, 1875.

The PRESIDENT (PROFESSOR BABINGTON) in the Chair.

On the Formation of Mountains on the hypothesis of a liquid substratum. *By* Rev. O. FISHER, F.G.S.

This paper was a sequel to one read in December, 1873, in which it had been shewn that, upon the supposition that the inequalities of the earth's surface have been formed by contraction of its volume through cooling, they are too great to be so accounted for if the earth has cooled as a *solid* body. In the present communication it was therefore assumed that there is a *liquid* layer beneath the cooled crust. After discussing several of the hypotheses of geologists regarding the formation of continental areas and ocean-basins, and of mountain-ranges, an enquiry was made regarding the form which a flexible crust would take, if it rested in corrugations on a liquid substratum. The answer arrived at was, that a section carried across the corrugations would approximately present at the lower surface a series of equal portions of circular arcs, concave upwards, and arranged end to end in a festoon-like manner.

Their common radius would be $2\frac{\rho}{\sigma}c$, where ρ, σ are the densities of the crust and liquid respectively, and c the thickness of the crust. The horizontal pressure would be zero at the highest points.

In applying this result to the crust of the earth, it was admitted that it can give only a very approximate solution of the problem. Owing to the defect from flexibility, and from absolute fluidity in the substratum on which it rests, the conditions assumed would not be strictly fulfilled, and they would also vary from place to place, so that no uniform result could be expected. Nevertheless it seems tolerably certain that

the forms of the corrugations would be of the above general character, and that they would not consist of long-phased rolling undulations with flattened anticlinals, but rather of cusp-like elevated ridges, with lateral profiles somewhat circular. The horizontal pressure, though not actually evanescent, would be small at the highest, and greatest at the lowest points of the corrugations.

The radius of the curves which define the sides of the elevations, as given by the above expression, being constant, any additional lateral compression could not be met by an increase in the curvature of the corrugations; so that there would be a tendency to accumulate material about the anticlinals. This circumstance would account for the plications observed on the flanks of mountain-ranges. For the tendency to heap the material together in such situations would be met by its descending superficially by its own weight, until it attained the angle of repose, and in so doing the strata would become plicated. Such seems a more probable account of the plications, which are often on quite a small scale, than that they have been formed directly by the general compression of the crust.

Causes were suggested which might tend to lessen the compression in the neighbourhood of anticlinals, and admit of the extrusion of steam and lava from below. In connection with this point it was argued that the permanent state of fusion of the lava in certain volcanic vents, such as Stromboli and Kilauea, can be due to nothing else than the passage of intensely heated vapours through them; whence it would follow that any place in the earth's crust, which is not sufficiently firmly constituted to prevent the passage of steam at a high tension, might be sufficient to originate a volcano.

It was admitted that the larger features of continental plateaux and oceanic depressions had been so far left unexplained.

March 8, 1875.

Professor STOKES in the Chair.

The following communications were made by Mr W. T. KINGSLEY,

(1) *On the cause of the "wolf" in the Violoncello.*

Mr KINGSLEY said that the "wolf" occurs somewhere about the low E or E flat, and was attributed to the finger-board having the same pitch, so that the finger-board becomes as it were a portion of the string stopped down on it and vibrates with it: if this is the true cause, the "wolf" cannot be got rid of, but may be placed at such a pitch between E and E flat as to occur on a note rarely used; also by thickening the neck of the finger-board, the extent of discursion in the vibration may be made less.

The MASTER OF ST CATHARINE'S COLLEGE remarked that a different explanation of the phenomenon was given by M. Savart, which was to this effect. The old Italian makers constructed the violoncello of such dimensions that the mass of air included within the instrument resonates to a note making 85·33 vibrations in a second, a number which then represented the lowest F on the C string, but which now, owing to the rise of pitch since the beginning of the 18th century, nearly represents the note E immediately below it. Savart's theory was that notes half a tone above or below this E will cause beats between the vibrations of the string and those of the mass of included air. It seemed quite possible that the mass of air contained in the instrument should be capable of controlling the vibrations of the whole instrument, but not that the vibrations of the finger-board alone (as Mr Kingsley suggested) could do this. For the sound, technically called the "wolf," is an actual

check to the whole vibration of the violoncello, producing not merely beats, but a baying sound, destitute of the freedom of vibration which characterizes other notes.

But a great objection to the above explanation is this experiment. On an Italian instrument, the upper D on the 4th or lowest string is the imperfect note. But when the same note is elicited from the 3rd string, the note is perfectly resonant. This peculiar effect seems then to depend upon the point of the finger-board which is pressed. It is also well known that the "wolf" can be modified by an alteration of the position of the sound-post. As an explanation, we may conceive that the whole framework of the violoncello vibrates like a stretched string, producing its fundamental, with a series of overtones, and that a nodal line passes through the point of the finger-board, pressure upon which produces the "wolf," and that thus all vibrations being destroyed except those which have a node at the point of pressure, this peculiar tone is elicited.

Mr TROTTER said that if Mr Kingsley's explanation of the cause of the "wolf" was the true one, it was to be expected that it should be produced when a certain note was sounded upon one string and not upon another. The fingering would be different in the two cases, and the note to which the finger-board responded would vary with the point touched by the finger.

(2) *A description of the Instruments used in sounding some of the Lakes in the Snowdon District, and an account of the results obtained.*

Mr KINGSLEY gave a description of the Plummet, Registering Apparatus and Protractors used by him in sounding several of the deep lakes in the Snowdon district last June.

The plummet is a modification of the deep-sea plummet now generally used, the principal alteration being in the appli-

cation of a heavy gouge to aid in bringing up specimens of the bottom.

The recording apparatus is a modification of the paying-out apparatus used for laying deep-sea telegraph cables.

The protractors are diagonal telescopes mounted on bars revolving on vertical axes, and having fiducial edges radiating from the centres of the axes.

One protractor is placed at each extremity of the base on a horizontal table, on which is strained a sheet of drawing paper; the telescopes are first collimated with each other, and then a line is drawn by the fiducial edges on each sheet of paper; the boat with the surrounding apparatus is followed by the two observers at the protractors, and when a signal is given, a line is ruled and numbered by each observer; finally the two papers are placed so as to have the lines of collimation in coincidence and the centres at the scale distances apart; then by looking through the papers and pricking the intersections of the corresponding lines, the positions of the boat are laid down on two maps.

In practice this is all done easily, and no particular skill is needed in the observers with the protractors.

The results obtained shewed that the bottoms of these lakes are comparatively flat, the greatest depths being reached at a short distance from the shore on the cross section, and occurring also nearer to the upper end of the lake than to the lower: the forms of the bottoms correspond in a remarkable manner with the set that would be given to glaciers descending into the hollows in which the lakes lie; and Mr Kingsley believed them to have been formed by the action of glaciers during the extreme cold or penultimate glacier epoch; because in one case, that of Llyn Cawlyd, the lake lies almost on a watershed, where no glacier could now form, but which was a depression forming a lateral outflow from the great glacier that at one time filled the whole hollow between the Glydyrs and Carnedds;

during the last glacier epoch most of these hollows were again filled with ice to a great height, but these last glaciers were comparatively small.

Mr Kingsley especially dwelt upon the difficulty of disentangling the scattered moraine from the drift, and also of distinguishing between the striations belonging to the two cold epochs.

Professor HUGHES, in a discussion with Mr Kingsley, explained and defended Professor Ramsay's theory of the glacial erosion of lake basins, but was not prepared to go so far as that author in his application of it. He said we wanted some more definite information as to the shearing force of ice: there was a limit to the possible depth to which ice could descend and pass out without being embayed. The observations in "roche moutonnée," where the surrounding ice corresponded to the sides of the rock-basin, shewed that ice could descend to a considerable depth without being embayed; and it had to be shewn in each separate case whether it was possible or probable that ice had scooped out, or been embayed in, any particular depression. He did not think there was evidence to shew that glacial conditions were synchronous in the Northern and Southern hemispheres, or even in Europe and America.

Mr HILL described a method by which a "camera obscura" may be employed to fix the position of the boat taking soundings.

Professor MAXWELL stated that piano-wire furnished the best means of suspending the sounding-plummet. He mentioned some other improvements in the methods used for taking soundings.

Mr BONNEY said that the facts collected by Mr Kingsley were very valuable, because the need of careful and accurate soundings was so much felt in discussions as to the origin of lake-basins. He was himself of opinion that the erosive force of glaciers was but small, and that they had not excavated

such lakes as those of Switzerland and Italy. From his own investigations in the Alps and Britain he was of opinion that a glacier could only erode a rock-basin under certain exceptional circumstances. After describing these, he stated that he was inclined to refer the rock-basins about Snowdon to the later period of glaciation. He also thought that such causes as change in the eccentricity of the earth's orbit, the effects of precession, and the alteration of sea and land, were more likely to have caused the glacial period than any variation in the sun's heat.

Mr ELLIS enquired how Mr Kingsley obtained the true depth when wind was blowing, and therefore the line not vertical, owing to the drifting of the boat.

April 19, 1875.

The PRESIDENT (PROFESSOR BABINGTON) in the Chair.

(1) *On the Mode of Formation of the Alimentary Canal in Vertebrata. By* F. M. BALFOUR.

The author stated that the simplest type of vertebrate development was that exhibited by Amphiòxus, and that all the complicated types found amongst other vertebrates were to be looked on as derivatives from this single type.

He shewed that in Amphioxus the alimentary canal was formed by a simple invagination; that in the Frog the invagination had ceased to be symmetrical and single as in Amphioxus, but had become unsymmetrical and had acquired other peculiar characters.

In the Selachian's development, which was to be looked upon as the type most allied to that of the Frog, the invagination was no longer present, but traces of it still remained. The

other features of development were exaggerations of what occurred in the Frog.

From the Selachian to the Bird the author pointed out that there was a wide gap which he could not satisfactorily fill up.

The author, in conclusion, drew attention to the food-yolk in the eggs of most vertebrates, which he said was to be looked upon as the most important agent in producing the modifications which he had described.

(2) *On the Physiological Action of Jaborandi. By* J. N. LANGLEY, St John's College.

May 3, 1875.

The PRESIDENT (PROFESSOR BABINGTON) in the Chair.

On a method of introducing a Current into a Galvanometer Circuit. By Mr PIRIE, Queens' College.

Mr PIRIE said that electricians had often to work with currents far too strong for their galvanometer. He mentioned various methods in use for checking the swing of the needle; but contended that an easily made and easily used controller for rough work was a desideratum. He described an instrument in the form of a continuously varying shunt, in which a moving connection was obtained by a tube filled with mercury sliding on a wire of suitable resistance. This form of connection was first used by Mr Barrett of Dublin.

With the aid of Mr GARNETT, the Demonstrator of Physics, Mr Pirie shewed that a very good connection was obtained by this means; and subsequently, that the instrument described gave a control over the movements of the needle in a galvanometer whose resistance was not too different from its own.

May 17, 1875.

The PRESIDENT (PROFESSOR BABINGTON) in the Chair.

The PUBLIC ORATOR made a communication

On the place of Music in Education as conceived by Aristotle (Politics v. [VIII.] cc. 3—7),

of which the following is an abstract:—

Gymnastic, Grammar and Drawing, considered as branches of education, have direct practical utilities; but it might be doubted, Aristotle says, what is the use of Music. Three objects might be assigned to the study of music: *παιδεία*, discipline; *παιδιά*, pastime; *διαγωγή*, the rational employment of leisure. On further inquiry we see that (leaving *παιδιά* out of account) the serious uses of music are these three—*παιδεία*, *διαγωγή*, and *κάθαρσις*, the purification of the emotions. I. *παιδεία*. The disciplinary value of music is (*a*) artistic, as training the perceptions, and so preparing *διαγωγή*: (*b*) moral, as establishing the faculty of rejoicing aright—*ἐθίζουσα δύνασθαι χαίρειν ὀρθῶς*. For, while forms or colours are merely symbols (*σημεῖα*) of character or feeling, musical sounds may be images (*ὁμοιώματα*); and pleasure in the imitative expression will create sympathy with the feelings imitated. But does music, as a part of early training, imply the power of performing upon any instrument? Aristotle holds that it does, since a certain measure of practical knowledge is necessary to make a competent critic: only, in order to guard against *τὸ βάναυσον*, the pursuit of executive skill must be limited by two things; first, the study of music must not interfere with other studies; secondly, the body of the citizen must not be unfitted for war or for those exercises which befit free men; the study of music

must stop short of what is τεχνική. II. διαγωγή. As the practical is subordinate to the speculative reason, so work is subordinate to rest; and the aim of education is to teach men, first, how they shall procure, next, how they shall use, leisure. Here, then, is the reason of the place held by music in the mature life of the normal citizen—it is one of the noblest and most elevating employments for leisure—ministering, in that quality, to two special purposes,—the culture of the intelligence (φρόνησις), first, by relaxation, then by a gentle exercise of the critical faculty in alliance with the imagination;—and the purification of the emotions. III. κάθαρσις. In the Poetics Tragedy is described as δι' ἐλέου καὶ φόβου περαίνουσα τὴν τῶν τοιούτων παθημάτων κάθαρσιν. Four principal explanations of 'katharsis' have been suggested:—(1) that 'moderating' of the emotions which might arise from familiarity with such objects as excite them: (2) 'chastisement of the *bad* passions'—an explanation which is at variance with Aristotle's language, since it excludes pity and terror themselves from the number τῶν τοιούτων παθημάτων: (3) 'the separation from pity and terror of what is disagreeable in such emotions when excited by *real* objects, and not, as in Tragedy, by fiction';—a view to which it may be objected that the work of κάθαρσις is manifestly something gradual, and, in its effect, lasting, not something confined to a momentary impression; it is a 'healing' of the soul: (4) 'the correction and refinement of the passions.' Twining says: 'the doctrine, therefore, of Aristotle...would perhaps only amount to this—that the habitual exercise of the passions by works of imagination in general of the serious and pathetic kind (such as Tragedies, Novels, &c.) has a tendency to soften and refine those passions when excited by real objects in common life.'

This view seems essentially modern. It may be doubted whether the idea of '*softening, refining*,' &c. had anything to do with the notion of κάθαρσις. Rather, probably, it means

'clarification'—*i.e.* presentment in typical clearness, with everything accidental or confusing withdrawn—in that *Heiterkeit* and *Allgemeinheit* which, as Winckelmann says, are the two characteristics of the Greek ideal. As in Sculpture, as in Tragedy, so in Music, this process would be a means, not necessarily of softening, but often of intensifying, by the simple and concentrated expression of feelings or issues from which the vulgar, the spurious, the petty, the falsely sentimental have been detached. What Tragedy does in the sphere of action, *πρᾶξις*, this Music does in the sphere of that normal imitation, *ἠθικὴ ὁμοίωσις*, which is its own. Aristotle's view as to the universal moral importance of music as an element in education seems to be enforced by some of those phenomena of the present day which shew how a repressed and uneducated sensibility may become ungovernable; though, since music has been set on a really scientific basis, his plea that it is necessary to be a performer in order to be an intelligent listener has no longer its Greek validity.

May 31, 1875.

THE PRESIDENT (PROFESSOR BABINGTON) in the Chair.

The following paper was read by Mr F. M. BALFOUR, of Trinity College.

On the Segmental Organs of Vertebrates.

The author stated that the recent investigations of Professor Semper and himself had led to the discovery that in the selachians the kidneys were developed from a series of primitively independent structures. Each of these was a tube opening at one end into the body-cavity and ending blindly at the other. These tubes corresponded in number with the proto-

vertebral segments. As development proceeded, the blind end of the most anterior of these became elongated, and gradually acquired a connection with each of the posterior tubes in succession, and finally at its posterior extremity opened to the exterior. The author then entered into further details as to the changes which these parts subsequently underwent, and attempted to demonstrate the homologies between the kidneys of selachians and those of the higher vertebrates.

He concluded by pointing out that the tubes he had described bore such a striking resemblance to the segmental organs of annelids that in his opinion the identity of the two structures was certain. It followed from this (1) that the ancestry of vertebrates was to be looked for in the annelids; (2) that the vertebral segments of the vertebrates were to be looked upon as similar to those of annelids, and not, as had sometimes been said, as due to a secondary segmentation.

NEW FELLOWS, &c.

During the Academical years 1873—1875, the following New Members of the Society were elected.

Honorary Members.

Mar. 16, 1874.	Col. J. T. WALKER, R.E., F.R.S.
April 27, „	Prof. A. T. ANGSTROM. Upsala, (since deceased).
„ „	M. CHEVREUIL. Paris.
„ „	M. OTTO VON STRUVE. Pulkova.
„ „	Prof. W. E. WEBER. Göttingen.
Feb. 22, 1875.	Dr T. ANDREW. Belfast.
„ „	Dr F. C. DONDERS. Utrecht.
„ „	W. K. PARKER, F.R.S. Hunterian Professor.
„ „	Sir W. R. GROVE, M.A., F.R.S.
„ „	Prof. J. C. POGGENDORF. Berlin.

Fellows.

Oct. 26, 1874.	Prof. COWELL, M.A., Corpus Christi College.
Nov. 16, „	OSBERT SALVIN, M.A., Trinity Hall.
„ „	G. J. ROMANES, M.A., Caius College.
Feb. 8, 1875.	Rev. A. ROSE, M.A., Emmanuel College.
„ „	F. H. NEVILLE, M.A., Sidney Sussex College.
Feb. 22, „	Rev. D. B. BANHAM, M.A., Caius College.
„ „	J. G. RICHARDSON, B.A., Trinity College.
May 17, „	H. F. BANHAM, M.A., St John's College.
„ „	D. BURGESS, B.A., Corpus Christi College.
May 31, „	G. CHRYSTAL, B.A., Corpus Christi College.

Associates.

Nov. 3, 1873.	Mr A. DECK.
Mar. 16, 1874.	Mr H. BAXTER.
Feb. 22, 1875.	Dr ARMITSTEAD.
„ „	Mr BOWES.

(PART XVI.)

PROCEEDINGS

OF THE

Cambridge Philosophical Society.

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ANNUAL GENERAL MEETING,

OCTOBER 25, 1875.

OFFICERS, &c. ELECTED FOR THE ENSUING YEAR.

President: Prof. MAXWELL.

Vice-Presidents: Prof. MILLER.
Mr MUNRO.
Prof. C. C. BABINGTON.

Treasurer: Dr CAMPION.

Secretaries: Mr J. W. CLARK.
Mr C. TROTTER.
Mr J. B. PEARSON.

New Members of Council.

Prof. NEWTON. Mr BONNEY. Mr H. M. TAYLOR.

NEW FELLOWS ELECTED, OCTOBER TERM, 1875.

Oct. 18. Prof. DEWAR.
Nov. 1. A. M. MARSHALL, B.A., St John's College.
F. M. BALFOUR, B.A., Trinity College.
H. N. MARTIN, B.A., Christ's College.

ERRATA (No. XV.)

P. 355, l. 8, *for* "ones" *read* "axes."
P. 376, last line, *for* "classification" *read* "clarification."

October 18, 1875.

THE PRESIDENT (PROFESSOR BABINGTON) in the Chair.

The following communication was made to the Society:

On some Fresh Observations of the Water-holes on the Gorner Glacier. By MR TROTTER.

The attention of the Society was called last year to certain water-holes on the Gorner Glacier associated with hummocks of ice on their southern edges (first observed by the speaker in 1863). (See Proceedings, Oct. 19, 1874.)

The holes as they appeared at the latter part of the season were oval, with their longer axes pretty exactly east and west, the larger axis about double of the smaller, the depth on an average nearly double the longer axis, the longer axis varying from about 1' to 6' or 8' long. The holes as usual had gravel at the bottom, and had usually a hummock of ice at the southern side, the height of which was often nearly equal to the longer axis in the smaller holes, somewhat less in proportion in the larger ones.

The larger axis was sometimes parallel, or nearly so, to the veined structure, sometimes cut it at a greater or less angle, so that the holes were clearly independent of the veined structure, and seemed to be clearly a meridian phenomenon.

The speaker had the opportunity of observing these holes last summer, about the end of June, when they were much less perfectly formed. The surface of the glacier was covered in places with new snow, and in others the winter's snow was imperfectly melted. Some of the holes, however, were fairly

developed, though of course not so deep as they were later on in the year. Others were of much less regular form: some had the hummock on the south side well developed; others had no perceptible hummock, but seemed to have more or less of a raised margin all round; others seemed to show a slight hummock on the north as well as the south side.

The explanation of the phenomenon which was suggested was as follows:—A collection of gravel gives rise to an irregular or roughly circular shallow water-hole, the water being at first at a considerably lower level than the edge of the hole, formed in part of snow and soft ice.

(1) Towards noon the sun's rays are incident upon the surface at a small angle, a comparatively small portion are reflected, and the radiations which enter the water are for the most part absorbed before reaching the wall of the hole, and the resulting heat is carried to the bottom by the descending current of dense water, whose temperature has been raised above the freezing point. This melts ice at the bottom and deepens the hole. On the other hand, towards morning and evening the angle of incidence is larger, a larger proportion of the rays are reflected and strike upon any portions of the east and west boundaries of the hole which are above water. The rays which enter the water have a shorter path to traverse in water before reaching the wall, and therefore will reach and melt it in a greater proportion. The east and west walls will thus be more melted than the south wall, and the hole gradually assumes its oval shape with its longer axis east and west.

(2) The ice in the neighbourhood of the holes is for the most part of very rough and irregular surface, very pure, and with small bright crystalline faces inclined in all directions. Consequently a considerable portion of the rays incident on any portion of it, and there reflected, will strike another portion of the surface, so that the wasting of any portion of the surface is due not only to the rays primarily incident on that portion,

but also to those incident upon it after reflection at another place. On the other hand, rays falling upon the water will be either absorbed or reflected regularly, so as to pass clear of the ice, except possibly the actual wall of the hole. Hence the ice in the immediate neighbourhood of the hole will receive less radiation, and therefore be less melted than the rest of the surface, and there will be a tendency to the formation of a raised rim surrounding the water-hole.

(3) As fast however as this rises, those portions of it which are on the north, east, and west sides of the hole will be melted, as they will at some part of the day receive the sun's rays not only on their upper surface, but on the vertical face towards the hole. The east and west sides however will be most attacked, for reasons given above in (1).

The final result will therefore be an oval hole with its major axis east and west, with a marked hummock of ice on its south side, and sometimes traces of one to the north.

(4) The most serious difficulty in the way of this explanation seems to be in the local nature of the phenomenon. Why is it not produced wherever there is a level surface of glacier? The ice where the phenomenon is conspicuous is of a peculiar soft nature, full of minute air-bubbles, which give it an unusually white appearance. Ablation probably takes place rapidly over the surface, so that phenomena depending upon differential ablation are conspicuous. The peculiar kind of irregularity of the surface would favour the action described in (2).

[*Communicated Nov.* 15, 1875.]

Since the notice of my paper read on Oct. 18th, 'Further remarks on the water-holes of the Gorner Glacier,' was published, my attention has been called to a passage in Agassiz' 'Nouvelles études sur les Glaciers,' &c., Paris, 1847, p. 101—2, in which a similar phenomenon is described as having been observed by Dr Ferdinand Keller. The account of the phe-

nomenon observed by Dr Keller on the Aar Glacier so closely resembles that given by me of the water-holes on the Gorner Glacier, that there can be no doubt that they refer to the same phenomenon, and that therefore it was first noticed by Dr Keller, and described by him in 1847.

I cannot however think that Dr Keller's explanation of the phenomenon is satisfactory. He speaks of the holes as semicircular with the arc towards the north, and attributes the greater depth of the northern portion of the hole to the longer time for which the sun will fall upon the gravel on the north portion of the bottom.

This does not explain the much more striking phenomenon, the east and west elongation of the hole. Moreover, the holes are so deep in the latter part of the summer that the gravel at the bottom must be in shade all day. No explanation is given of the accompanying hummocks of ice, which are spoken of as if they existed before the water-hole, whereas my observations of last summer make it clear that they are formed subsequently. I therefore still adhere to the explanation given in my paper, which seems to me to explain all the phenomena.

October 25, 1875.

THE PRESIDENT (PROFESSOR BABINGTON) in the Chair.

The following communication was made to the Society:

On Herwart ab Hohenburg's Tabulæ Arithmeticæ προσθαφαιρεσέως universales, Munich, 1610. *By* J. W. L. GLAISHER, F. R. S.

The title more at length is "Tabulæ arithmeticæ *προσθαφαιρεσέως* universales, quarum subsidio numerus quilibet, ex multiplicatione producendus, per solam additionem: et quotiens

quilibet, e divisione eliciendus, per solam subtractionem, sine tædiosâ & lubricâ Multiplicationis, atque Divisionis operatione, etiam ab eo, qui Arithmetices non admodum sit gnarus, exactè, celeriter & nullo negotio invenitur. E museo Ioannis Georgii Herwart ab Hohenburg...Monachii Bavariarum...Anno Christi, M.DC.X.", and the book is a very large and thick folio. It contains a multiplication table up to 1000×1000, the thousand multiples of any one number being given on the same page; and there is an introduction of seven pages, in which the use of the table in multiplying numbers containing more than three figures, and in the solution of spherical triangles, is explained.

Very little information about the work is to be obtained from the mathematical bibliographers and historians. Heilbronner (*Hist. Math.* 1742, p. 801) gives the title not quite correctly, and adds "Docet in his tabulis sine abaco multiplicationem atque divisionem perficere." Kästner (*Gesch. der Math.* 1796—1800, t. iii. p. 8) quotes the title from Heilbronner and his remark, and adds that the latter could not have known Herwart's method, or he would have described it. He remembers to have read somewhere that the book contained a number of tables of products, arranged by factors, like a great multiplication table. Scheibel (*Einl. zur math. Buch.* 1775, t. ii. p. 417) gives the title-page correctly, and explains the method of using the table when the number of figures in the multiplier or multiplicand exceeds three, and concludes with the remark, "So viel von diesem ungeheuren Folianten, den man bloss zur Curiosität und seiner Seltenheit wegen, in einer mathematischen Büchersammlung aufbewahret." Montucla (*Hist. des Math.* t. ii. p. 13) gives a description of the mode of using the table, remarking that but for the invention of logarithms it might have been of use to calculators, supposing the labour of searching for the products in so large a folio not to be more fatiguing than the direct performance of the work. Murhard

(*Bibl. Math.* 1797—1804, t. ii. p. 199) gives the title correctly, and marks it with an asterisk to show that he has seen the work himself. Rogg (*Bibl. Math.* 1830, p. 142) merely has, "Hohenburg, Gregor (sic) Herwardt ab, tabulæ arithmeticæ προσθαφαιρεσεως universales, 1610." Neither Weidler (*Bibl. Ast.* 1755), Deschales (*Cur. seu Mund. Math.* 1690), Lalande (*Bibl. Ast.* 1803), nor Delambre (*Hist. de l'Ast. mod.* 1821), mention the work; but there is a reference to it in Leslie's Philosophy of Arithmetic (2nd edit. 1820, p. 246). In his article on tables in the English Cyclopædia (1863) De Morgan wrote, "The table goes up to 1000 × 1000, each page taking one multiplier complete. There are then a thousand odd pages, and as the paper is thick, the folio is almost unique in thickness. There is a short preface of seven pages, containing examples of application to spherical triangles. It is truly remarkable that while the difficulties of trigonometrical calculation were stimulating the invention of logarithms, they were also giving rise to this the earliest work of extensively tabulated multiplication. Herwart passes for the author, but nothing indicates more than that the manuscript was found in his collection. The book is excessively rare, a copy sold by auction a few years ago was the only one we ever saw." Graesse (Trésor de livres rares, 1859—1867) says that by the book the use of logarithms was first spread in Germany, which is of course erroneous.

Herwart was Chancellor of the Palatinate of Bavaria, and published several other works (the most complete list is in the Bodleian Catalogue), among which his "Ludovicus IV. imperator defensus (Munich, 1618—19)" is the best known. In the *Biographie Universelle* it is described as still useful for the history of Germany; and Scheibel speaks of Herwart as "der berühmte Staatsmann und Geschichtschreiber."

While I was engaged in preparing the report of the British Association Committee on mathematical tables, I endeavoured

without success to find something beyond what is quoted above about the table; but the hope is there expressed that, considering the attention so large a work must have received from contemporary mathematicians, some information might still be gained with regard to the calculator of the table, his objects, &c.

I recently found a correspondence of six letters between Herwart and Kepler, which took place at the end of 1608, with regard to the table, and which throws light upon these points. The letters are printed in Dr Frisch's 'Joannis Kepleri Astronomi opera omnia' (t. iv. pt. II. pp. 527—530, 1863).

In the first letter, dated September 13, 1608, Herwart writes, "Ich hab bisher in Multiplicatione et Divisione sonderbare geschriebene praxin gebraucht, dadurch ich den numerum ex quavis multiplicatione productum, per solam additionem, und den Quotienten ex divisione resultantem per solam subtractionem (absque tediosa multiplicationum et divisionum operatione) gefunden." He states that J. Prætorius and others who have seen it recommend him to have it printed, and he adds that if he had not had this method, on account of his continual occupations and because he is not a good calculator, he should long ago have had to give up all mathematics that required calculation. He sends a specimen page of the table, the use of which he explains, and he prays Kepler to give him his opinion on the matter without delay.

Kepler replies on October 18, 1608, and remarks that 1000 pages will make a large volume, which the computer will often not have at hand. He suggests that short precepts on the solution of triangles should be added, as Herwart's table would often be preferable to the 'προσθαφαιρεσις Vitichiana,' which is too elaborate to be retained in the memory, confuses sines and their complements, &c. Besides, the reasons for the operations are hidden in work, "At si multiplicemus et dividamus simpliciter, tunc videmus quid agamus; et possunt varietates trian-

gulorum talibus præceptis comprehendi, quæ memoria retineri facile possunt." Kepler then gives a synopsis of the sixteen cases of the solution of spherical right-angled triangles.

Herwart writes on November 5, and says he had found that the triangles could be solved better by help of his table than by prosthaphæresis, so that Kepler was quite right. He mentions that he has not seen Vitichius. As it is usual to prefix a 'splendid title' to books, that they may sell, he suggests the following, "Nova, exacta, certa et omnium facillima ratio Arithmetices, per quam numerus ex multiplicatione productus sine operatione multiplicationis per solam additionem, et quotiens ex divisione resultans absque operosis ambagibus divisionis per solam subtractionem cujuscunque, etiam maximæ summæ, etiam ab eo qui arithmetices non admodum sit gnarus, citius quam ulla alia ratione invenitur." He then prays Kepler to send soon his advice as to how the table should be entitled.

Not receiving an answer on December 2, he writes a short letter to Kepler again asking for a reply, and suggesting that perhaps it was not prudent 'so speciosum titulum tantillæ rei zu præ figiren.' But in the mean time a letter from Kepler had been received, and, writing on December 5, Herwart explains that he does not expect an answer to his last letter, and that he understands that Kepler has no objection to the title, but thinks it ought to be shortened. He cannot understand the meaning of Kepler's advice, 'Græca compositio imploranda, sed exercito (*sic*),' and asks for an explanation.

In the last letter of the correspondence, dated December 12, 1608, Kepler explains that as the title seemed long, he had advised that it should be shortened by the composition of two Greek words as *ἐρυσιπελας*, *ριψοκινδυνος*, and as a suitable word did not occur to him he had suggested that some one practised in Greek should be consulted. But perhaps a good idea has occurred to himself. *Σεισαχθεια ἀριθμητικη*. "Nosti enim, *χρεων αποκοπας* sic dici. Inest vocabulo et emphasis et pro-

prietas et similitudinis gratia, quia me Hercule novas tabulas introducis, et uno ictu liberas computatores debitis multiplicandi, et dividendi inextricabilibus.' But he hopes that this title will not offend Maginus on account of his 'Tabula tetragonica' [Venice, 1592]. At the end he adds the postscript, "Titulus igitur talis: **Σεισαχθεια**, sive Novæ Tabulæ, quibus Arithmetici debitis inextricabilibus multiplicandi et dividendi liberantur, ingenis, tempori, viribusque ratiocinantis consulitur."

It is thus proved that the table was printed from a manuscript which Herwart used himself, and which very likely he had had made. The correspondence is of interest, as the table, regarded simply as a multiplication table, has never been surpassed in extent, and has only been equalled by Crelle's *Rechentafeln*, first published in 1820, in two volumes, and which, now sold in one volume folio, is one of the best known and most used tables. Scheibel and others who have ridiculed Herwart's project were only right in so far as the great size of the work renders it unmanageable, as the use of a multiplication table up to 1000×1000 has, in spite of logarithms, been found to be both practicable and convenient. Herwart's work is very rare, but there are copies in the British Museum, the Bodleian Library, and the Graves Library at University College, London. It was this last (not quite perfect) copy which, through the kindness of Professor Henrici, I was enabled to exhibit to the meeting.

With regard to the word prosthaphæresis, it is well known that the prosthaphæresis of the orbit was the angle subtended by the eccentricity at the planet, and De Morgan explained the use of the word on the title-page thus: "Prosthaphæresis is a word compounded of prosthesis and aphæresis, and means addition and subtraction. Astronomical corrections, sometimes additive and sometimes subtractive, were called prosthaphæreses. The constant necessity for multiplication in forming proportional parts for the corrections, gave rise to this table, which

therefore had the name of its application on the title-page." But the prosthaphæresis referred to seems most likely a method of solving spherical triangles, in which the product of two sines, or of a sine and cosine, &c., is avoided by the use of formulæ, such as $\sin a \sin b = \frac{1}{2}\{\cos(a-b) - \cos(a+b)\}$. This explains all Kepler's allusions to prosthaphæresis, and as Herwart proposed as the chief use of his tables to solve spherical triangles by direct multiplication without previous transformation, as set forth in his introduction, it justifies completely the use of the word on the title-page.

Wittich was for a short time an assistant of Tycho Brahe, and his method of prosthaphæresis appears to have been a method of solving triangles so as to avoid multiplications by means of formulæ, such as that just written, but I hope to examine the matter more fully. Laplace (*Jour. de l'école polyt.* Cah. xv. t. viii. 1809), referring to the same formula,

$$\sin a \sin b = \tfrac{1}{2}\{\cos(a-b) - \cos(a+b)\},$$

remarks that "cette manière ingénieuse de faire servir des tables des sinus à la multiplication des nombres, fut imaginée et employée un siècle environ avant l'invention des logarithmes" (see Brit. Ass. Tables Report, p. 23, 1873).

November 1, 1875.

PROFESSOR BABINGTON, VICE-PRESIDENT, in the Chair.

The following communication was made to the Society:

On Aristotle's notion of 'Right-Handedness'. By Mr PEARSON.

After referring to the paper by Dr Hollis on this subject, communicated last year (Nov. 30, 1874), the speaker stated that he had been led by Aristotle's great reputation to enquire

what his views on the subject might have been. Partly from a perusal of much that Aristotle has written on the subject, but mainly from the new Index by Prof. Bonitz, he gave a *résumé* of the passages bearing on the subject: these passages seemed to shew that Aristotle considered, (1) that the right hand or side was naturally the source or origin of motion, (2) that in nearly all living creatures capable of motion it is the better or stronger side, (3) but that while the heart is always the origin of vitality, it is in the human race only set towards the left side of the body; in all other living creatures it is in the centre of the body or trunk (*ἐν μεσῷ κεῖται τοῦ ἀναγκαίου σώματος*). And though it may be a fair question how far Aristotle was misled by the preferential use of the right hand by the human race to attribute an excellence in the right side to the animal world, there can be no mistake about the distinct language in which he does so.

The passages referred to by Mr Pearson were as follows:

Περὶ Ζώων Μορίων, II. 2; III. 3, 4, 5; IV. the whole section, especially n. 8.

Περὶ Ζώων Ἱστορίας, I. 17; II. 1. 17.

Περὶ Ζώων Πορείας, XIX.

Nicom. Eth. V. 7 (10).

**Magna Moral.* I. 34.

Politics II. (9) 12.

**Problems* V. 37; VI. 5; XXXI. 12, 13.

De Respir. 16.

Plato, *Legg.* VII. 795.

Macrobius VII. 14.

Some observations were added about the terms in which Mr Lewes in his work on Aristotle (1864) criticizes some errors into which that writer has fallen in his works on Natural History, while it was admitted that he is probably right in considering that there is no reference in Aristotle's writings to the anatomical examination of any but animal subjects: though the

fact that Macrobius (VII. 14) ascribes to Erasistratus and Herophilus, two celebrated physicians of the succeeding generation, the practice not only of dissection but of vivisection of human bodies, shews, if the story is true, that public opinion could not have been quite unprepared for it.

Mr Pearson also referred to a passage in the *Encyc. Britan.*, art. *Comp. Anatomy* (§§ 202—205), ed. 1810 (but not occurring in later editions), in which the preferential use of the right hand is discussed, and ascribed to a natural peculiarity in the form of the sub-clavian and carotid arteries on that side: and in which it is stated that a similar preference for the right side may be traced in some dogs if not in horses. The speaker said, however, that he would not answer for the existence of such a preference himself in those animals, nor in the lion and camel, to which Aristotle (and Pliny after him) especially ascribe it. He concluded by exhibiting a lobster; a kind of shell-fish of which the right claw is distinctly larger and stronger than the left, as is specially mentioned by Aristotle (*οἱ καρκινοὶ...τὴν δεξίαν ἔχουσι χηλὴν μείζω καὶ ἰσχυροτέραν...*).

In conclusion an opinion was expressed that the preferential use of the right arm might be due to a natural shrinking from the use of the side nearest the heart, or perhaps a natural wish to protect it as being, to our own sensations at least, the seat of vitality. At any rate such a use must be accepted as a fact, whether the more strictly anatomical reasons given in the former paper were correct or not, and the difficulty of coming to a conclusive view on the subject was suggested as a reason why the point is so little discussed, at any rate in the more popular and simpler treatises on anatomy: nor had the speaker been successful in finding much information on another question: viz., the true position of the heart in animals, Aristotle's view on this point being very decided, while the practice of dissecting them was evidently common in his time: but still he ventured to think that the situation of the centre of gravity of the human heart

to the left of the centre of the body had been a sufficient determining cause in favour of the preferential use of the right arm by our species.

Mr NEVILLE GOODMAN said that while he had listened with pleasure to the elucidation of Aristotle's ideas on the causes of right-handedness he thought these indicated that the inductive method was preferable to the speculative. These ideas shewed how possible it was for an ingenious man with the great repertory of nature before him to adduce many facts to support any theory once formed. No doubt there were many facts which would support the idea that the right side had a preferential motor function. In addition to those named, the whole order of Gasteropods might be quoted, in which in the vast majority of instances the opening of the generative organs, the vent and the respiratory chamber, were on the right side. In fact an ordinary snail or whelk exhibited the phenomenon of an excessive development of the right side, which excessive development had the effect of thrusting that side continuously over the other so as to result in the dextral helix. There were, however, many exceptions to this rule, as the *Funis Contrarius* of the Red Crag and numbers of existing shells, Clausiliæ, &c. Flat-fish (Pleurovectidæ), the only animals of the vertebrate type markedly asymmetrical in their organs of relation, were by no means constant in having the right side of stronger motor function than the other; the upper, coloured, and more convex and muscular side being in many cases on the left. In the sole the developed side is usually the right, in the turbot usually the left.

With regard to the prior motion of the right side from a state of rest he had narrowly observed horses' paces and thought there was no ground for this supposition. The leading leg in the horse's canter became so purely from training. Both dogs and young horses constantly change the leading leg when running unrestrained. All the figures in Egyptian Art had

their left leg advanced in conformity with Aristotle's remarks, but he had regarded this attitude as purely conventional.

The heart of all mammals was of like asymmetrical position to that of man, and its position on the left side was more apparent than real; the butt end being directed to the right, and its apex or lower end to the left. The beating of the apex on the left side was caused by rhythmical distension of the aortic arch, which thus became periodically straightened, and in relaxing the apex fell back on the left side of the thorax. It was quite possible and probable that, owing to the depression of the human thorax, (i.e.) its greater lateral than fore and aft diameter as compared with the compression of the same part in the lower animals, the apex of the heart might be thrust more to the left side, but he thought that the centre of gravity of the heart occupied a similar position in both cases.

With regard to the cause of right-handedness, as it could not be due to external conditions, it seemed reasonable to attribute it to the asymmetry of the internal organs, but he thought that the stomach, whose cardiac end was on the left side and which when distended with food bulged to that side, had a greater claim for consideration. The greater proximity of the right hand to the source of arterial blood through the innominate trunk, was also worthy of consideration. The greatest argument against accounting for right-handedness by any of these methods was that while cases of reversal of the whole viscera were known the individuals thus characterized were not left-handed. Right-handedness was so universal throughout the human race, that he thought it could not be accounted for by early education, the customs of the world in all conventional matters being so various. If due neither to asymmetry of the organs of nutrition nor to education, it must be an inherited instinct, accidental in the sense of being due to a cause which has now no bearing on the species. Such a meaningless and persistent habit would go far to prove the unity of the human

race. It would be very interesting to observe whether the *Quadrumana* shewed any preference in the use of the right hand. He had observed the smaller monkeys, and concluded that they seized and wrought indifferently with either hand, but the larger anthropoid apes he had not observed.

November 15, 1875.

PROFESSOR BABINGTON, VICE-PRESIDENT, in the Chair.

The following communications were made to the Society:

(1) *On the behaviour of Nucleus during Segmentation. By* F. M. BALFOUR.

The following observations were made upon the eggs of Scyllium and Pristiurus. At a late stage of the segmentation of these eggs most of the segments contain nuclei, but in some of them there is to be seen in the place of the nucleus a peculiar body. This has the shape of two cones with their bases in apposition. In each cone a series of striæ radiate from the apex to the base; and between the two is an irregular row of granules. From the apex of the cone there further diverge into the protoplasm of the cell a series of lines. The author regards these peculiar bodies as metamorphosed nuclei in the act of dividing. He points out that the simple division of the nucleus, as well as its complete disappearance, accompanied by the formation of two fresh nuclei, are well-authenticated modes of behaviour of the nucleus during cell-division. These two processes can only be connected on the supposition that in the second case the two fresh nuclei are formed from the matter of the old nucleus. The author considers that there exist in Selachians modes of behaviour of the nucleus intermediate

between the two extremes mentioned above, and points out that in the peculiar striation of the body he described there are indications of the streaming out of its matter into the surrounding protoplasm; while on the other hand it never completely vanishes. It therefore affords an instance where part of the matter of the nucleus divides and part streams out into the protoplasm of the cell to be again collected to assist in the formation of two fresh nuclei. The author further states that he has found other bodies intermediate between the cone-like bodies mentioned above and true nuclei; and regards these also as nuclei in the act of division, where a still larger bulk of the protoplasm of the nucleus becomes divided and a smaller part rises with the surrounding protoplasm.

(2) *On the effects of Upas Antiar on the Heart.* *By* M. Foster, M.A., F.R.S.

The author recording the movements both of the ventricle and the auricles of a frog's heart (Rana temporaria), within the body, by means of two delicate levers, observed in addition to the well-known phenomena of antiar poisoning, a marked slowing of the rythm in the later stages of the action of the poison. The prolongation of each systole was also distinctly marked, especially in the case of the auricles, which, much distended in consequence of the partial occlusion of the contracted ventricle, caused the lever resting on them to make an enormous excursion at each systole. So long as any beat was capable of being recorded by the lever resting on the ventricle, the ventricular systole occurred in its proper sequence. Though the whole rythm often became irregular, the phases of each cardiac cycle remained regular.

Repeating the experiment of Schmiedeberg (*Beit. zu Anat.*

u. Phys.: also *Festgabe*, C. Ludwig *Gewidmet*, p. 222), the author found that when the ventricle had apparently ceased to beat, forcible distension of its cavity with a normal solution brought back a temporary series of pulsations; but this restoration is possible only within a narrow range of time, and as Schmiedeberg himself seems to admit, cannot be regarded as shewing that the poison's chief action consists in preventing the normal muscular relaxation following upon each systole.

Repeating Neufeld's observation (*Stud. Phys. Inst., Breslau*, III. p. 97) the author found that strong solutions of potassium cyanide would sometimes restore the beat for a short time—but in this case also the phase at which this could be effected was very transient and very frequently failed, and inasmuch as such solutions are capable of stimulating muscular tissue directly, he was led to the conclusion that the restoration when obtained is not due to any relaxing action of the cyanide but to its chemical stimulation of the cardiac muscles.

When the vagus is stimulated in the earlier stages of antiar poisoning, inhibition is obtained as usual, but is followed by a somewhat lengthened period in which the beats are both more rapid and more forcible.

When the antiar has produced such an effect on the heart that its beats are exceedingly feeble and hardly capable of being recorded, this secondary action of vagus stimulation becomes exceedingly marked, the pulsations during its continuance being as forcible or even more forcible than normal, and at the same time rapid.

Lastly, a stage of poisoning may be witnessed when the ventricle is apparently at rest (i.e. not pulsating at all as far as the eye can judge, though of course in the contracted state so characteristic of antiar), where stimulation of the vagus produces no inhibition (for there is no beat to stop) but is followed by a lengthened series of often very vigorous and rapid pulsations. The author could not satisfy himself that *during* the

stimulation of the vagus any *relaxation* of the contracted ventricle took place, but on this point he is not sure.

These results at first sight seem identical with the phenomena observed by Schmiedeberg in nicotin poisoning (Ludwig's *Arbeiten*, 1870, p. 41), and explained by him as due to *accelerator* fibres in the vagus of the frog.

The author is unable to accept this explanation:

1. Because both inhibition and secondary action fail when atropin is given with the antiar (the antiar otherwise acting as usual).

2. Because a similar secondary action may be seen in all cases of inhibition not only of the frog's heart but also of the mammalian heart in which the accelerator fibres are supposed to run *not* in the vagus.

3. Because a similar secondary action may be seen in the snail's heart after inhibition by direct application of the interrupted current, and in certain conditions of the snail's heart may be witnessed when the inhibition cannot be detected.

The author regards the secondary action as being what for want of more precise knowledge he would call "a reaction" following the direct action of the vagus. And considering that antiar acts essentially, or at least primarily, on the muscular tissue of the heart, the peculiar prominence of this reaction in antiar poisoning may be taken as indicating on the one hand that the effects of antiar are especially favourable to this reaction, and on the other that the vagus nerve brings about inhibition by acting directly on the muscular tissue itself—a view which is supported by other facts.

November 29, 1875.

THE PRESIDENT (PROFESSOR CLERK MAXWELL) in the Chair.

The following communication was made to the Society:

On the temperatures observed in a deep boring at Speremberg near Berlin, as given in a report of a paper by Professor Mohr, of Bonn, in 'Nature' of October 21, 1875. *By* MR O. FISHER.

The greatest depth recorded is 3390 feet. The temperatures are given in Reaumur's scale. The author shewed that the equation

$$v = -\frac{251}{10^8}x^2 + 0{\cdot}012982\,x + 7{\cdot}1817,$$

in which v is the temperature, and x the depth, exactly represents the temperature curve. This curve would give a maximum temperature of

$$40^\circ{\cdot}7532 \text{ R., or } 123^\circ{\cdot}6947 \text{ Fah.,}$$

at a depth of 5171 feet. If there was no cause to disturb the temperature, it ought to conform to a straight line, given by the above equation altered by omitting the term in x^2. Consequently a cause was sought which would change such a straight line to the parabolic form. The first cause examined was a change in the conductivity of the strata depending on the depth, and it was found that a law, which would make the conductivity vary inversely as the distance of any point above the level of greatest temperature, would account for the observed facts. But it was argued that such a law was entirely improbable.

The next cause examined was the effect of the descent of water through the strata, and the author believes that this circumstance will account for the observed temperatures.

It was remarked that the results of this investigation make it appear, that the true law of underground temperature would be better obtained from borings of moderate than of very great depth, because the disturbance of the temperature curve from the rectilinear form is greater the further we descend.

April 19, 1875[1].

THE PRESIDENT (PROFESSOR BABINGTON) in the Chair.

On the Physiological Action of Jaborandi. By Mr J. N. LANGLEY.

[*Abstract.*]

The preparations used are

(1) The alcoholic extract of the crushed Jaborandi leaves.

(2) The glycerine solution of this extract evaporated to dryness.

The results of experiments point to there being more than one active principle.

Injected subcutaneously there is one striking difference in the action of Jaborandi on the Frog and the Rat. In the former it causes convulsive movements with occasional tetanic spasms, in the latter it acts as a narcotic. Death in one case is probably more immediately caused by the stopping of the heart's beat; in the other by paralysis of the respiratory centre.

In the Frog convulsive movements are noticeable if any part of the spinal cord be left intact, but not otherwise.

Reflex action is greatly depressed.

Jaborandi has little or no effect on nerves, their endings in striated muscle, or on striated muscle itself.

If an arterial blood-pressure tracing be taken of a Mammal, and Jaborandi injected into a vein, it causes

(1) A fall in the blood-pressure.

(2) A slowing of the pulse.

(3) Generally a flattening of the respiratory curves.

[1] Omitted, by accident, from the last Number of the "Proceedings."

(1) The character of this depends very largely upon the method of injection, the rapidity of fall being directly as the rapidity of introduction into the blood.

The blood-pressure continues lower for two or three hours—the longest time during which any experiment was carried on. It may be lowered to a very considerable extent, less than one-half.

The fall is largely due to dilatation of the small blood-vessels, since

(*a*) After injection of Atropin the blood-pressure does not rise though the slowing of the heart is removed.

(*b*) After injection of Atropin the blood-pressure is still further reduced by a fresh injection of Jaborandi, though the heart-beat rate remains the same.

(*c*) When the Jaborandi is carefully and slowly injected, the tracing of the fall of blood-pressure is not recognisable from that obtained by stimulation of the central end of the Depressor.

The fact that after the blood-pressure has been very considerably lowered by Jaborandi, stimulation of the central end of the depressor gives a further lowering, points to its being due, not to a central paralysis of the vaso-motor centre, but to some local action: moreover Jaborandi causes dilatation in the blood-vessels of the Frog's web after section of the sciatic.

(2) The slowing is not due to a stimulation of the cardio-inhibitory centre, since it takes place in the Frog after complete destruction of the brain and spinal cord, and in the Frog after section of both vagi.

In the Frog there is an increased susceptibility to inhibition of the heart by vagus stimulation after giving Jaborandi.

With a moderate dose and after some time, stimulation of the sinus venosus still produces inhibition, though with a larger dose, and after some time, it no longer does.

Atropin removes the slowing rapidly and effectually, the

heart of a frog which has ceased beating may be caused to beat again by putting a few drops of dilute solution of Atropin on it.

As the heart regains its rapidity of beat, the inhibitory power of the pneumogastric is lost.

Since the slowing takes place after a large dose of Curari, it probably acts more peripherally than the endings of the vagus nerves.

Some curves obtained after a dose of Jaborandi very strongly suggest that they are due, not to the ordinary effects supposed to give the respiratory curves, but to a rhythmic increase in the force of the heart-beat.

Some tracings show a want of correspondence between the 'respiratory curves' of the blood-pressure and the respirations: it is hoped that further inquiry into this and some other deviations from the normal tracings may throw some further light on the causes of the rhythmic variations in blood-pressure.

(3) The flattening of the respiratory curve is probably very largely, if not entirely, a mechanical effect of the slowing of the heart-beat; a like flattening is obtained by a weak stimulation of the pneumogastric.

Whether Jaborandi, apart from the alcohol or glycerine in which it is dissolved, can produce a local stasis, is doubtful but not impossible.

Jaborandi causes in Mammals at first a quickening of the respiration, then a slowing, then a paralysis of the respiratory centre: in one rabbit (under chloral) in which respiration had been stopped by a slow injection of Jaborandi, it was restored after two or three minutes of artificial respiration.

With regard to the effect on the secretions, only a few experiments have been made more than mentioned in a Preliminary Notice last February. In Mammals the lachrymal and salivary secretion are always increased, but in no case has it been observed to cause sugar in the urine.

(PART XVII.)

PROCEEDINGS

OF THE

Cambridge Philosophical Society.

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32

Feb. 14, 1876.

THE PRESIDENT (PROFESSOR CLERK MAXWELL) in the Chair.

The following communication was made to the Society:

On the effect of the constant current on the Heart. By MR FOSTER and MR DEW SMITH.

Feb. 28, 1876.

THE VICE-PRESIDENT (PROFESSOR BABINGTON) in the Chair.

The following communication was made to the Society:

On Bow's method of drawing diagrams in graphical statics, with illustrations from Peaucellier's linkage. By J. CLERK MAXWELL, M.A., *Professor of Experimental Physics.*

The use of Diagrams is a particular instance of that method of symbols which is so powerful an aid in the advancement of science.

A diagram differs from a picture in this respect, that in a diagram no attempt is made to represent those features of the actual material system which are not the special objects of our study.

Thus when we are studying the internal equilibrium of a particular piece of a structure or a machine, we require to know its shape and dimensions, and the specification of these may often be made easier by means of a drawing of the piece.

But when we are studying the equilibrium of a framework composed of such pieces jointed together, in which each piece acts only by tension or by pressure between its extremities, it is not necessary to know whether a particular piece is straight or curved or what may be the form of its section. In order, therefore, to exhibit the structure of the frame in the most elementary manner we may draw it as a skeleton in which the different joints are connected by straight lines. The tension or pressure of each piece may be indicated on such a diagram by numbers attached to the line which represents that piece in the diagram. The stresses in the frame would thus be indicated in a way which is geometrical as regards the position and direction of the forces, but arithmetical as regards their magnitude.

But a purely geometrical representation of a force has been made use of from the earliest beginnings of mechanics as a science. The force is represented by a straight line drawn from the point of application of the force, in the direction of the force, and containing as many units of length as there are units of force in the force. The end of the line is marked by an arrow-head to show in which direction the force acts.

According to this method each force is drawn in its proper position in the diagram which represents the configuration of the system. Such a diagram might be useful as a record of the results of calculation of the magnitude of the forces, but it would be of no use in enabling us to test the correctness of the calculation. It would be of less use than the diagram in which the magnitudes of the forces were indicated by numbers.

But we have a geometrical method of testing the equilibrium of any set of forces acting at a point by drawing in series a set of lines parallel and proportional to these forces. If these lines form a closed polygon the forces are in equilibrium. We might thus form a set of polygons of forces, one for each joint of the frame. But in so doing we give up the principle of always

drawing the line representing a force from its point of application, for all the sides of a polygon cannot pass through the same point as the forces do.

We also represent every stress twice over, for it appears as a side of both the polygons corresponding to the two joints between which it acts.

But if we can arrange the polygons in such a way that the sides of any two polygons which represent the same force coincide with each other, we may form a diagram in which every stress is represented in direction and magnitude, though not in position, by a single line, which is the common boundary of the two polygons which represent the points of concourse of the pieces of the frame.

Here we have a pure diagram of forces, in which no attempt is made to represent the configuration of the material system, and in which every force is not only represented in direction and magnitude by a straight line, but the equilibrium of the forces is manifest by inspection, for we have only to examine whether each polygon is closed or not.

The relations between the diagram of the frame and the diagram of stress are as follows:

To every piece in the frame corresponds a line in the diagram of stress which represents in magnitude and direction the stress acting on that piece.

To every joint of the frame corresponds a closed polygon in the diagram, and the forces acting at that joint are represented by the sides of the polygon taken in a certain cyclical order. The cyclical order of the sides of two adjacent polygons is such that their common side is traced in opposite directions in going round the two polygons.

When to every point of concourse of the lines in the diagram of stress corresponds a closed polygon in the skeleton of the frame, the two diagrams are said to be reciprocal.

The first extensions of the method of diagrams of forces to

other cases than that of the funicular polygon were given by Rankine in his *Applied Mechanics* (1857).

The method was independently applied to a large number of cases by Mr W. P. Taylor, a practical draughtsman in the office of the well-known contractor Mr J. B. Cochrane. I pointed out the reciprocal properties of the diagram in 1864, and in 1870 showed the relations of this method to Airy's function of stress and other mathematical methods.

Prof. Fleeming Jenkin has given a number of applications of the method to practice, *Trans. R. S. E.*, Vol. xxv.

Cremona[1] has deduced the construction of the reciprocal figures from the theory of the two linear components of a wrench.

Culmann in his *Graphische Statik* makes great use of diagrams of forces, some of which, however, are not reciprocal.

M. Maurice Levy in his *Statique Graphique* (Paris, 1874) has treated the whole subject in an elementary and complete manner.

Mr R. H. Bow, C.E., F.R.S.E., in a recent work *On the Economics of Construction in relation to Framed Structures* (Spon, 1873), has materially simplified the process of drawing a diagram of stress reciprocal to a given frame acted on by any system of equilibrating external forces.

Instead of lettering the joints of the frame as is generally done, or the pieces of the frame as was my own custom, he places a letter in each of the polygonal areas enclosed by the pieces of the frame, and also in each of the divisions of the surrounding space as separated by the lines of action of the external forces.

When one piece of the frame crosses another, the point of intersection is treated as if it were a real joint, and the stresses of each of the intersecting are represented twice in the diagram of stress, as the opposite sides of the parallelogram which repre-

[1] *Le figure reciproche nella statica grafica* (Milano, 1872).

sents the forces at the point of intersection. Thus the point V in figures 1 and 3, p. 412, is represented by the parallelogram $BCDE$ in figure 2, and the point A in figure 2 is represented by the parallelogram $PRQS$ in figures 1 and 3.

Peaucellier's linkage consists of the four equal pieces forming the jointed rhombus $PQRS$ together with two equal arms OS and OR.

When these arms are longer than the sides of the rhombus the linkage is said to be positive; when they are shorter the linkage is said to be negative.

When Peaucellier's linkage is employed as a machine it is acted on by three forces, applied respectively at the fulcrum O, and the two tracing poles Q and S.

These three forces, if in equilibrium, must meet in some point T. We may therefore suppose them to be stresses in three new pieces OT, QT, ST, which will complete the frame.

Let us suppose that both O and T are outside the rhombus, and that OS intersects PT in the point V, and let us apply Bow's method to construct the diagram of stress reciprocal to this frame.

If we letter the areas as follows, putting

A for the rhombus $PRQS$,
B for the triangle PSV,
C for the triangle OTV,
D for the quadrilateral $ORPV$,
E for the quadrilateral $QSVT$,
and F for the space outside the frame,

then, in the diagram of stress, the stresses of the four sides of the rhombus will meet in A, and since the opposite sides of the rhombus are parallel, the lines EA and AD will be in one straight line, and the lines BA and AF will also be in a straight line.

Also since in the frame the pieces OR and OS are equal, the angles ORP, PSV are equal, and the corresponding angles

Fig 1

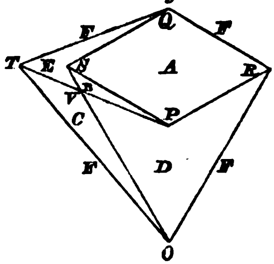


Fig 2

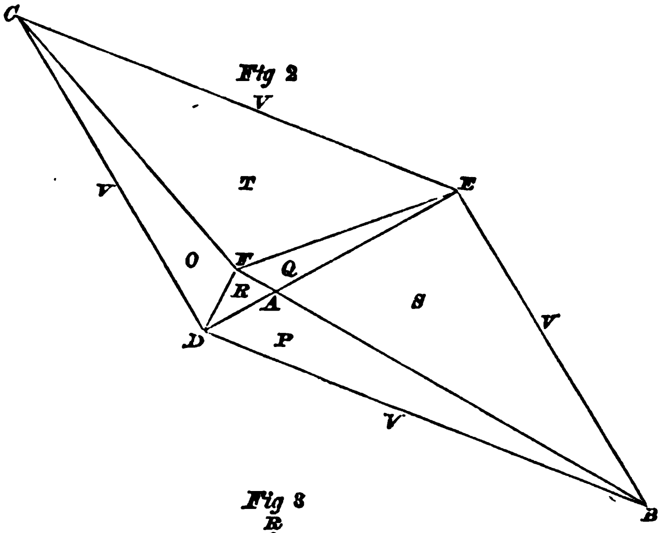
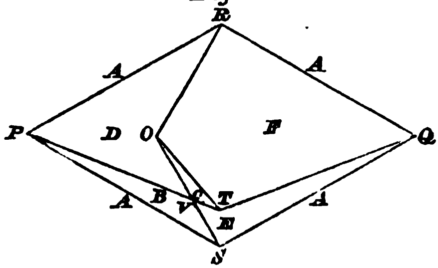


Fig 3



FDA, *ABE* must be equal, and therefore the quadrilateral *BEFD* can be inscribed in a circle, and therefore the angles *FEA*, *DBA* are equal, and the corresponding angles in the frame *TQS*, *SPV* are equal, and therefore *PT* is equal to *QT*.

If, therefore, O is in one diagonal of the rhombus, T must be in the other diagonal.

The diagram of stress is completed by drawing *EC* parallel to *BD*, and *DC* parallel to *BE*, and joining *FC*.

This diagram therefore consists of a parallelogram *BDCE*, a diagonal *ED*, a point F in the circle passing through *FBD*, and four lines drawn from F to the angles of the parallelogram.

If we now begin with the diagram of stress, and proceed to construct a frame reciprocal to it, the form of the frame will be different according to the cyclical direction in which the sides of the rhombus *PRQS* are lettered. If in the one case we have the points O and T both outside the rhombus as in fig. 1, in the other O and T will both be within the rhombus as in fig. 3. The stresses in the corresponding pieces of fig. 1 and fig. 3 are all equal if they are equal in any pair of them.

If in the frames represented in fig. 1 and fig. 3, we consider that the pieces *OS* and *TP* cross one another at V without intersecting, we have six points O, P, Q, R, S, T joined by nine lines. Now in general if p points in a plane are joined by $2p-3$ lines the figure is simply stiff, that is to say the form of the figure is determined by the lengths of the lines, and there are no necessary relations between the lengths of the lines.

But in Peaucellier's linkage the length of any line, as *OT*, is determined when those of the other eight are given. For if a is the length of a side of the rhombus, b the length of either arm *OR* or *OS*, c the length of either arm *TP* or *TQ*, then if $OT=d$,

$$d^2=b^2+c^2-a^2.$$

Hence if any one of the nine pieces of the linkage be removed, the motion of the remaining eight will be the same as

before, and a given stress in any one of the nine will produce stresses in each of the other eight which are determinate in magnitude when the configuration of the linkage is given, though they alter during the motion of the linkage.

March 13, 1876.

THE PRESIDENT (PROFESSOR CLERK MAXWELL) in the Chair.

The following communication was made to the Society:

On a set of Lunar Distances. *By* MR PEARSON.

In this paper I propose to discuss the anomalies exhibited by a set of Lunar distances which I took under rather peculiar circumstances last autumn. The sky was perfectly clear, with the exception of a light cloud touching the sun, at the commencement: and the horizon quite open. The position of observation was, by the ordnance survey, as near as possible Lat. 52°. 7'. 13" N., Long. 56^s. (14') E. The instrument with which they were taken is the same prism-circle, 6 inches in diameter, by Pistor and Martins, of Berlin, which was shewn on a previous occasion at a meeting of the Society, and with which I took the observations given pp. 351—354, and pp. 357—359 of this vol. of the *Proceedings*.

It will be well first to consider the condition of the instrument itself, as it may be suggested that the source of the anomalies is to be found here. Being graduated all round, the mean of the opposite readings has in all cases been taken. Between these there is, in the first case, a discrepancy between the opposite readings of two divisions of the vernier, or 40" about; for (2) we have a discrepancy of 35"; (3), (4), (5), and (7) give the same readings on both sides of the arc; (6) gives a discrepancy of 20". At 101° and at 102° there is no discrepancy between the opposite readings: so that it is clear that

the error in the first two cases is only casual: and also, what is of more importance, that the centering is very nearly accurate; and though it is just possible that the large error in the measured distance in the two first cases is due to bad graduation, it is clear that the fault extends to both sides of the instrument. The index-error (additive) at the zero-point having remained constant for some months at about 10", this amount has been added in all cases. Having thus shewn that the anomalies can only be due to a fault in the graduation on the corresponding opposite sides of the instrument, such as can only be accurately estimated by a long series of observations and comparison with other instruments, I will proceed to give the elements of the observations themselves.

	G.M.T.	L.M.T.	☉ app. alt.	☉ true alt.	☽ app. alt.	☽ true alt.
	h. m. s.	h. m. s.	° ′ ″	° ′ ″	° ′ ″	° ′ ″
(1)	4.32.26	4.33.22	6.37.20	6.29.38	5.43.32	6.29.37
(2)	4.39. 4	4.40. 0	5.40.20	5.31.23	6.13.13	6.59.50
(3)	4.41.18	4.42.14	5.20.58	5.11.38	6.22.52	7. 9.40
(4)	4.44. 3	4.44.59	4.57.24	4.47.18	6.34.50	7.21.50
(5)	4.46.35	4.47.31	4.35.33	4.24.45	6.47.39	7.34.52
(6)	4.53.26	4.54.22	3.36.45	3.23.45	7.13.40	8. 1.12
(7)	4.55.22	4.56.18	3.20.13	3. 6.28	7.21.10	8. 8.50

	Obs. dist. of cents. of ☉ & ☽.	Reduced dist. of do.	Error in time.	Error in arc.
	° ′ ″	° ′ ″	m. s.	′ ″
(1)	101.26.50	101.21.27	5.34	2.36
(2)	101.29. 8	101.24.36	5.31	2.34
(3)	101.30.40	101.26.26	3.35	1.38
(4)	101.32.20	101.28.30	1.59	0.56
(5)	101.32.10	101.28.42	4.46	2.13
(6)	101.35.20	101.32.58	2. 8	1. 0
(7)	101.36. 0	101.33.58	1.44	0.49

As the G. M. T. was obtained by comparing the watch with a carefully rated chronometer immediately after the observation, I am certain that it is not more than 5^s to 10^s, at the most, out.

We have here a set of 7 observations, the moon to the left of the meridian about 2 hours, the sun to the right about 4½ to 5 hours.

The moon at first low. App. alt. 5°. 43'.

The sun at the same time higher. App. alt. 6°. 37'.

The moon at the last had risen 1°. 40'. App. alt. 7°. 21'.

The sun „ „ fallen 3°. 45'. „ 3°. 20'.

The fourth observation I suspect, but this I need not discuss.

Now, firstly, the mean of the first two gives the place of observation $5^m. 32\frac{1}{2}^s$ to the East of its true place, *that* of the last two diminishes this error to 55^s; but as, taking the mean of the first two, the 4th and 5th, and the 6th and 7th, the errors marked by them are in the proportion of 5½, 3½, 3⅓, 2, nearly; a mean of them all cannot fairly be taken.

Secondly: all these observations give an earlier Greenwich, and therefore local time, than the true time; because, allowing that the distances tabulated in the *Nautical Almanac* are correct, they make the distances between the sun and moon less than they ought to be, as the moon was receding from the sun at the time.

Query. Can this be the error of the *instrument?*

It cannot be the *Index Error,* commonly so called, because I repeatedly examined this under the most favourable circumstances, by taking the distance between the two opposite (horizontal) edges of the sun.

It cannot be from simple *false centering,* because, at this point of the graduated arc, there is so small a discrepancy between the readings on the two sides, and that not continuous.

In order to discuss the matter properly, I will refer to some other examples.

First set of examples, which I have taken since.

The moon and Venus (Mar. 1, p.m.) both to *right* of meridian (moon's H.A. 2½ hours, Venus's 4½ about) give

Observed dist. 31°. 39'. 35" Computed dist. 31°. 41'. 15"	error in arc 1'. 40" (abt. 3^m. 8^s time); too small, as in the case of the sun, already given.

Also the sun, in the previous example; the planet *in this*, are to the right of the moon: but the moon, in this case, is at a much *greater* altitude.

But for three cases of *Pollux* and the *moon*, the moon in this case being to the right of the star, not to the left, as before, I obtained

	1.	2.	3.
Obs. dist.	32°. 38'. 15"	30°. 39'. 4"	30°. 33'. 46"
Comp. dist.	32°. 37'. 31½"	30°. 37'. 42"	30°. 32'. 59"
	43½"	1'. 22"	47"
	(abt. 1^m. 20^s time.)	(abt. 2^m. 20^s time.)	(abt. 1^m. 20^s time.)

In all these cases the *observed distance is too great;* but the moon and star in the 1st case were on opposite sides of the meridian; in the latter two cases only, the H.A. of the moon was considerable. With these I couple a case of Mars and the moon (H.A. of Mars 1^h. 50^m, of moon 3^h. 15^m), both to right of meridian; because the error agrees with that in the case of Pollux in a similar position; the computed distance (but not given in the N. A.) at a certain time (Jan. 29, 1876) being 23°. 29'. 13", and the reduced measured distance 23°. 30'. 6", or nearly 1' too great.

In another example (Sept. 7, 1875, p.m.), with the moon and sun very low (less than 10°), and the former almost on the meridian, I had an error of 1'. 44" arc, or 3^m. 41^s time. In this case again the observed distance was too small, and the sun, of course, to the *right* of the moon.

I may mention that Capt. Parry in fixing his Long. at Port Bowen 88°. 54'. 55" E.—73°. 13'. 39" N. Lat., made his Long. by 6 occultations of fixed stars to differ from the mean of all his observations by 4" only. But by

Moon's transits (a large number) ...	2'. 42".
By eclipses of Jupiter's satellites ...	2'. 40".
By chronometers........................	20".
By lunar distances	26".

Again, he took 620 sets (310 E., 310 W.), and the extreme error is as much as 32'. As 10 went to a set, it is possible that a part of the error arose from taking so many together.

Lastly, I will mention one very accurate observation of the moon and Jupiter, May 10, 1875 (dist. 81°. 30'. 30"), which gave the Long. of Emmanuel College 45^s E., or only 15^s too much. Here the observed distance is about 10", or one division of the vernier too large, but the error is too small to notice.

I may conclude by saying that in a few other cases of Lunars, when the moon has been nearly on the meridian, and at an altitude of at least 30°, I have not found any error except what might be properly ascribed to defects in my own power of observing, or a local error in the graduation of the circle; also that we may be sure that the large error in the first observations cannot well have been due to an error in the estimated amount of refraction, as the thermometer and barometer had been observed, Th. 48°, Bar. 29°. 9, about cancelling one another. (See on this point *Ast. Soc. Month. Not.* Vol. XXIII. p. 58; Zach. *Monat. Corresp.* XXVII. p. 341; Shortrede's Log. Tables (ed. 1844), Introd. p. 12; Brinkley in *Memoirs of R. I. A.* XIII. p. 170.) Nor has it escaped my notice that had the changes in the *reduced* distance (by Borda's formula, I believe, see *ante*, p. 358) agreed with those in the *observed* distance, the error which I have discussed would not nearly have amounted to so much.

March 27, 1876.

THE PRESIDENT (PROFESSOR CLERK MAXWELL) in the Chair.

The following communications were made to the Society:

On the relation of the Spinal Cord to the Tail in Mammals. By MR ANNINGSON.

After noticing the varying position of the spinal cord and its nerves at different ages in man, and quoting some anatomical works in reference to the length of the spinal cord and the position of its nerves in long- and short-tailed mammals respectively, the speaker proceeded to shew that some of the statements contained in the books were not quite in accord with the evidence of his own dissections. The facts which he wished to point out were (1) The constancy of a cauda equina and filum terminale in both long- and short-tailed mammals; (2) The superficial position attained by the filum terminale towards the end of the tail; (3) The general constancy in the absolute number of sacro-caudal nerves irrespective of the total number of sacro-caudal vertebræ; (4) The direct relation between the number of sacro-caudal spinal nerves and the number of ossified neural arches. He concluded by pointing out the relation the above facts might bear to the development of the tail in the individual and in the mammalian series.

On Vital Force. By MR H. F. BAXTER.

After a few preliminary observations the author proposes the following question, What is the nature of Vital Force? By vital force he means the *force* that is manifested during the growth, development, or evolution of organised bodies, both plants and animals. Can we associate this *force* with any other

known *form* of force? Do the actions, such as *secretion, nutrition* and *absorption,* which take place in an organised body, differ from those which occur in the laboratory of the chemist?

Reference is then made to the well-known conjecture and experiment of Wollaston which occurred so far back as 1806, and originated at the time Davy made his celebrated discovery of the decomposition of chemical compounds by means of the voltaic battery. In Wollaston's experiment, a weak solution of common salt was used and the soda made to transude through a membrane, the metals employed being zinc and silver, forming an elementary circle. "The efficacy of powers," says Wollaston, "so feeble as are here called into action, tends to confirm the conjecture that similar agents may be instrumental in effecting the various animal secretions which have not yet been otherwise explained." Wollaston's conjecture was so far true, but it is necessary to be acquainted with Faraday's views in regard to the *origin* of the power in the voltaic circle; the current not being the *cause* of the power, but a manifestation of the chemical action that is taking place. Allusion is then made to Becquerel's experiments to show that metals are not necessary to obtain current force; ordinary chemical actions, such as the combination of an acid with an alkali, will produce it.

The author then refers to the results of his own experiments with regard to *secretion,* first in the *stomach* and *intestines*; secondly with the *biliary secretion*; thirdly with *urinary secretion,* and fourthly with *mammary secretion.* In all these cases the electrode in contact with the *venous blood* flowing from the part was *positive* to the electrode in contact with the *secreted* product. To account for these results according to Becquerel's experiments we should be obliged to assume that the venous blood was acid to the secreted product, and not only that, but that just after the separation had taken place a *combination* occurred between the secreted product and the blood. As this reasoning

organic force be considered as a polar force also, as the essential conditions for current force exist in both cases.

Reference is next made to the results obtained during *lacteal absorption,* and those during *nutrition* in the muscular and nervous tissues, and to his experiments upon the roots and leaves in plants. In all these cases, although the results are not so decisive as in the case of secretion in animals, they nevertheless indicate that similar results may be obtained. The author then sums up with the following remarks. Let me now state the general conclusions that the results of all these experiments lead me to infer. The animal body is not a voltaic battery, nor an electro-magnetic machine, nor a steam-engine. I have been *comparing* the actions which take place in the voltaic circle and those which occur in the organised body, and find they are identical in all their essential conditions; that the existence of current force which is manifested in the one case exists also in the other; that the *direction* of the current depending upon the direction in which the *anion* and *cation* move in the circle is the same in both instances. If the manifestation of current-force be evidence of *polar* actions in one case so must it be in the other; if chemical force be polar so must organic force be polar. I am now talking of organic force, the title of my paper is vital force. Vital force and organic force to my mind are identical terms; vital force may perhaps be more comprehensive as embracing the action of the nervous system. We can have life without nerves, as in plants. But what do I mean by the term life? By life I mean *the series of changes which take place in organic matter resulting in the development, or evolution, of an individual organism.* By an individual organism I mean a *monad*, a *man*, an *annual plant.*

March 8, 1876.

THE PRESIDENT (PROFESSOR CLERK MAXWELL) in the Chair.

The following communications were made to the Society:

(1) *On the Friction attributed to the Ether.* *By* Mr W. M. HICKS.

In the Proc. Roy. Soc. Vols. XIV. XV. XXI. appear descriptions of some experiments of Stewart and Tait on the heating of a disk by rapid rotation in vacuo. The experiments were made with thin disks of aluminium and ebonite, the apparatus with a thermo-pile being placed inside a receiver and the receiver exhausted. On rotating the disk for 30″ or 40″ the pile showed a heating effect.

For aluminium disks of $\frac{1}{6}$ in thickness the effect on the pile corresponded to a rise of temperature of about ·85° F. and of $\frac{1}{40}$ in thickness to a rise of 1·7° F.

They showed that the heating effect is not due to either

1. Rotation under the Earth's magnetic force,
2. Nor conduction of heat from the bearings,
3. Nor to radiation or convection from the wheel-work,
4. Nor to vibrations of the disk.

In a second series of experiments, they found that the heating effect was due to two causes,

1. A residual gas effect,
2. An unknown effect.

This unknown effect they set down as due to etherial friction, but they arrive at this conclusion by a process of exhaustion, and hence they cannot be sure of having exhausted all other possible causes. And one cause, at least possible a priori, they do seem to have passed over.

The unknown effect to be explained has the following properties:

1. It is a surface-effect more deeply seated than the gas-effect.

2. It varies in the same proportion along the radius as the gas-effect (and is therefore probably proportional to the distance from the centre).

3. The quantity of heat produced in disks of different thicknesses appears the same, as the radiation was found to be proportional inversely to the thickness.

4. The effect is produced without perceptible diminution when the disk is covered with a chamois leather blind.

5. It is independent of the residual gas.

6. It is different for different disks.

Now the disk when rapidly rotating expands slightly, and consequently becomes cooled below the surrounding space. Hence during the time of rotation an equilibrium of temperature takes place and it becomes heated up towards its former temperature, when the rotation is stopped the disk shrinks to its former size and gives out the heat it had taken in whilst it was rotating. Now let us see how this explanation would satisfy the required conditions.

1. It is clearly a surface-effect when the rotation has not been continued too long, for the heat enters from outside and gets conducted inwards, and therefore it will be hotter outside than inside. It will be also more deeply seated than the gas-effect, for it is heated by radiation which affects a perceptible depth, while the gas-effect takes place on the surface only.

2. It can be shown that the displacement at any point of the disk is, when small, very nearly proportional to the distance from the centre, or $\xi = \mu r$. The strains are therefore $\frac{d\xi}{dr} = \mu$ and $\frac{\xi}{r} = \mu$ constant along and perpendicular to the radius. The

work done per unit of mass is therefore everywhere the same, and the temperature ought therefore to be uniformly raised all over the surface. This would imply that the gas-effect was uniform all over the disk, which is not probable. The explanation therefore does not seem to agree with experiment here, but unfortunately the experiment on which the law is based is rather unsatisfactory. In the first place, the temperatures were compared at two points only and distant from one another only $\frac{1}{8}$ radius; and secondly, the amount itself to be measured was very small.

3. The quantity of heat taken in would probably be the same for different thicknesses of the same kind of disk, for 40″ would not be long enough for even the thinnest disk to rise exactly to the surrounding temperature.

4. This would certainly be the case, though scarcely probable on the supposition of etherial friction.

5. It is clearly independent of the residual gas.

6. And it is evidently different for different disks. In fact, for india rubber it ought to give a cold effect.

If we consider what the etherial friction would be, it seems more probably due to a shearing force, separating the ether in the body from the ether in space than a true friction in the ordinary sense. If this were so, it would probably be to a great degree independent of the material of the disk; but still it is clear that different materials would be differently affected, though the effect might not depend on the polish of the surface.

The question could at once be settled by the following experiments.

1. There ought to be at first, when the disk is in motion, a temporary cooling effect. (The heating effect was observed only when the disks were at rest.)

2. The work done at any point is proportional to the square of the angular velocity, while for friction it would

clearly be proportional to the angular velocity directly. Unfortunately the experiments were always made with the same angular velocity.

From Thermodynamic considerations we can show that the rise of temperature due to compression is

$$\Delta t = \left(\frac{c}{c'} - 1\right)\frac{\epsilon}{\mu} - \frac{1}{Jc'}\frac{dE}{dm},$$

where $c . c'$ are specific heats at constant pressure and volume respectively, ϵ is the compression, μ the coefficient of expansion for heat, and $\frac{dE}{dm}$ the external work done per unit of mass.

It can be shown that the displacement at any point is given by

$$\xi = \frac{4\omega^2 a^2 m}{15\lambda} r = L\frac{m}{\lambda} . r, \text{ say,}$$

where λ = coefficient of resistance to form,

a = radius of disk,

m = mass of unit of volume.

Work done at any point per unit mass

$$= \frac{\delta E}{dm} = \lambda\left(\frac{d\xi}{dr}\Big|^2 + \frac{\xi}{r}\Big|^2\right)\frac{\delta v}{\delta m} = \frac{2m}{\lambda}L^2.$$

The compression $\epsilon = \frac{d\xi}{dr} + \frac{\xi}{r} = \frac{2m}{\lambda} . L,$

whence $$\Delta t = \left\{\left(\frac{c}{c'} - 1\right)\frac{2L}{\mu} - \frac{2L^2}{Jc'}\right\}\frac{m}{\lambda}.$$

In the experiments

$$\omega = 2500 \text{ rev. in } 30'' = \frac{500\pi}{3},$$

$$a = 7{\cdot}5 \text{ in.} = {\cdot}1905 \text{ metres nearly},$$

$$J = 424 \text{ gramme-metres.}$$

Substituting these values we find

$$\Delta t = \left\{ \left(\frac{c}{c'} - 1 \right) \frac{1010 \cdot 7}{\mu} - \frac{1204 \cdot 6}{c'} \right\} \frac{m}{\lambda}.$$

The units being gramme, metre, second, centigrade.

The experimental value of $\frac{c}{c'}$ for aluminium has not been found, but Prof. Maxwell has pointed out to me that Edlund has determined it for some other metals. So, though we are unable to calculate the amount of the heating for an aluminium disk, we may get an idea of its magnitude by taking some other metal. If we take silver we shall have

$$\frac{c}{c'} = 1 \cdot 0203,$$

$$c = \cdot 057,$$

$$\mu = \cdot 000057,$$

$$\lambda = 8481 \times 10^9 \text{ grammes per square metre},$$

$$m = 10 \cdot 4 \times \text{mass of cubic metre of water},$$

$$= 10 \cdot 4 \times 10^6 \text{ grammes}.$$

Substituting these we find for a silver disk under the conditions of the experiment,

Fall of temperature on expanding = ·4° C.

As the conductivity of silver is very high, the heat absorbed during rotation would be rapidly conducted inwards, and therefore after 40″ the disk will almost have risen to the surrounding temperature, and consequently on stopping the disk we should get the whole effect of ·4° C. showing itself. If we take into consideration the effect of the conduction at the end of 40″, the surface would be ·04° C. below the surrounding space, and therefore on stopping the disk the temperature observed ought to be ·36° C.

The order of magnitude of the effect is thus the same as

that of the experiments, and the explanation proposed seems sufficient to account for all the results.

If the heating is due to friction, the amount was shown to be about ·0006 lbs. per square feet, and that this would produce an alteration in the length of the day of not less than ·006″ in a century.

(2) *On the Equilibrium of Heterogeneous Substances.* *By* Prof. Clerk Maxwell.

The thermodynamical problem of the equilibrium of heterogeneous substances was first attacked by Kirchhoff in 1855, who studied the properties of mixtures of sulphuric acid with water, and the density of the vapour in equilibrium with the mixture. His method has recently been adopted by C. Neumann in his *Vorlesungen über die mechanische Theorie der Wärme* (Leipzig, 1875). Neither of these writers, however, make use of two of the most valuable concepts in Thermodynamics, namely, the intrinsic energy and the eutropy of the substance.

It is probably for this reason that their methods do not readily give an explanation of those states of equilibrium which are stable in themselves, but which the contact of certain substances may render unstable.

I therefore wish to point out to the Society the methods adopted by Professor J. Willard Gibbs of Yale College, published in the *Transactions of the Academy of Sciences of Connecticut*, which seem to me to throw a new light on Thermodynamics.

He considers the intrinsic energy (ϵ) of a homogeneous mass consisting of n kinds of component matter to be a function of $n+2$ variables, namely, the volume of the mass v, its eutropy η, and the n masses, $m_1, m_2 \ldots m_n$, of its component substances.

Each of these variables represents a physical quantity, the

value of which, for a material system, is the sum of its values for the parts of the system.

By differentiating the energy with respect to each of these variables (considered as independent), we obtain a set of $n+s$ differential coefficients which represent the intensity of various properties of the substance. Thus,

$\frac{d\epsilon}{dv}=-p$, where p is the pressure of the substance;

$\frac{d\epsilon}{d\eta}=\theta$, where θ is the temperature on the thermodynamic scale;

$\frac{d\epsilon}{dm_1}=\mu_1$, where μ_1 is the potential of the component (m_1) with respect to the compound mass.

Each of the component substances has therefore a potential with respect to the whole mass.

The idea of the potential of a substance is, I believe, due to Prof. Gibbs. His definition is as follows:—

If to any homogeneous mass we suppose an infinitesimal quantity of any substance to be added, the mass remaining homogeneous, and its eutropy and volume remaining unchanged, the increase of the energy of the mass, divided by the mass of the substance added, is the *potential* of that substance in the mass considered.

The condition of the stable equilibrium of the mass is expressed by Prof. Gibbs in either of the two following ways:

I. *For the equilibrium of any isolated system it is necessary and sufficient that in all possible variations of the state of the system which do not alter its energy, the variation of its eutropy shall either vanish or be negative.*

II. *For the equilibrium of any isolated system it is necessary and sufficient that in all possible variations of the state of the system which do not alter its eutropy, the variation of the energy shall either vanish or be positive.*

The variations here spoken of must not involve the transportation of any matter through any finite distance.

It follows from this that the quantities $\theta, p, \mu_1 \ldots \mu_n$ must have the same values in all parts of the mass. For if not, heat will flow from places of higher to places of lower temperature, the mass as a whole will move from places of higher to places of lower pressure, and each of the several component substances will pass from places where its potential is higher to places where it is lower, if it can do so continuously.

Hence Prof. Gibbs shows that if Θ, P, $M_1 \ldots M_n$ are the values of $\theta, p, \mu_1 \ldots \mu_n$ for a given phase of the compound, and if the quantity

$$K = \epsilon - \Theta\eta + Pv - M_1 m_1 - \&c. - M_n m_n,$$

is zero for the given fluid, and is positive for every other phase of the same components, the condition of the given fluid will be stable.

If this condition holds for all variations of the variables the fluid will be absolutely stable, but if it holds only for *small* variations but not for certain finite variations, then the fluid will be stable when not in contact with matter in any of those phases for which K is positive, but if matter in any one of these phases is in contact with it, its equilibrium will be destroyed, and a portion will pass into the phase of the substance with which it is in contact.

Thus in Professor F. Guthrie's experiments, a solution of chloride of calcium of 37 per cent. was cooled to a temperature somewhat below $-37°$ C. without solidification.

In this state, however, the contact of three different solids determines three different kinds of solidification. A piece of ice causes ice to separate from the fluid. A piece of the cryohydrate of chloride of calcium determines the formation of cryohydrate from the fluid, and the anhydrous salt causes a precipitation of anhydrous salt.

The phase of the fluid is such that K is positive for all phases differing slightly from its own phase, and its equilibrium is therefore stable, but for certain widely different phases, namely, ice, cryohydrate and anhydrous salt, K is negative.

If none of these substances are in contact with the fluid, the fluid cannot alter in phase without a transport of matter through a finite distance, and is therefore stable; but if any one of them is in contact with the fluid, part of the fluid is enabled to pass into a phase in which K is negative. The conditions of consistent phases are that the values of $\theta, p, \mu_1 \ldots \mu_n$, and K are equal for all phases which can coexist in equilibrium, the surface of contact being plane.

This was illustrated by Mr Main's experiments on co-existent phases of mixtures of chloroform, alcohol and water.

MONDAY, *May* 22, 1876.

THE PRESIDENT (PROFESSOR CLERK MAXWELL) in the Chair.

The following communication was made to the Society:

On Curvilinear and Normal Co-ordinates. By the Rev. J. W. WARREN, M.A. (Communicated by Prof. CAYLEY.)

THE Memoir refers partly to the general theory of curvilinear co-ordinates, partly to the special case of normal co-ordinates.

Taking (u, v, w) each of them a given function of the rectangular co-ordinates (x, y, z), so that a point is determined either by its rectangular co-ordinates (x, y, z) or by its curvilinear co-ordinates (u, v, w), and writing

$$dx^2 + dy^2 + dz^2 = (\mathrm{a, b, c, f, g, h})\,(du, dv, dw)^2,$$

and then, Ω being an arbitrary function of (x, y, z), or of (u, v, w),

$$\left(\frac{d\Omega}{dx}\right)^2+\left(\frac{d\Omega}{dy}\right)^2+\left(\frac{d\Omega}{dz}\right)^2=(A, B, C, F, G, H)\left(\frac{d\Omega}{du}, \frac{d\Omega}{dv}, \frac{d\Omega}{dw}\right)^2,$$

then $(\mathrm{a}, \ldots)$, $(A, \ldots)$ are given functions of the differential coefficients $\frac{dx}{du}$ &c., ... or $\frac{du}{dx}$ &c., that is of (x, y, z), or, what is the same thing, of (u, v, w), such that

$$\begin{aligned} & A : B : C : F : G : H \\ & = \mathrm{bc}-\mathrm{f}^2 : \mathrm{ca}-\mathrm{g}^2 : \mathrm{ab}-\mathrm{h}^2 : \mathrm{gh}-\mathrm{af} : \mathrm{hf}-\mathrm{bg} : \mathrm{fg}-\mathrm{ch},\end{aligned}$$

and

$$\begin{aligned} & \mathrm{a} : \mathrm{b} : \mathrm{c} : \mathrm{f} : \mathrm{g} : \mathrm{h} \\ & = BC-F^2 : CA-G^2 : AB-H^2 : GH-AF : HF-BG : FG-CH,\end{aligned}$$

and the theory of curvilinear co-ordinates is in fact a theory of the mutual relations of these coefficients $(\mathrm{a}, \ldots)$ and $(A, \ldots)$.

In Lamé's system of curvilinear co-ordinates where the surfaces $u=0$, $v=0$, $w=0$ are orthotomic, $\mathrm{f}=\mathrm{g}=\mathrm{h}=0$, and therefore also $F=G=H=0$: and the remaining coefficients correspond to Lamé's $\mathrm{h}, \mathrm{h}_1, \mathrm{h}_2, H, H_1, H_2$; viz. we have

$$H=\frac{1}{\mathrm{h}}=\frac{1}{\sqrt{A}}=\sqrt{\mathrm{a}}, \quad H_1=\frac{1}{\mathrm{h}_1}=\frac{1}{\sqrt{B}}=\sqrt{\mathrm{b}}, \quad H_2=\frac{1}{\mathrm{h}_2}=\frac{1}{\sqrt{C}}=\sqrt{\mathrm{c}},$$

and Lamé gives six differential equations of the second order satisfied by $\mathrm{h}, \mathrm{h}_1, \mathrm{h}_2$, or H, H_1, H_2, considered as functions of the variables which correspond to (u, v, w).

In the author's system of normal co-ordinates, u, v, w denote the normal distances of the point (x, y, z) from three given surfaces $u=0$, $v=0$, $w=0$ respectively: and the coefficients are then such that $A=B=C=1$. He obtains on this assump-

tion six differential equations of the second order satisfied by a, b, c, f, g, h considered as functions of (u, v, w); viz. the forms are

$$\frac{d^2b}{dw^2}+\frac{d^2c}{dv^2}-2\frac{d^2f}{dv\,dw}=\text{given function of first derived functions,}$$

$$\vdots$$

$$\frac{d^2g}{du\,dv}+\frac{d^2h}{du\,dw}-\frac{d^2a}{dv\,dw}-\frac{d^2f}{du^2}=\text{given function of do.}$$

$$\vdots$$

and as a consequence of these he obtains a seventh differential equation of the second order, symmetrical as regards the coefficients (a, b, c), (f, g, h), and the variables (u, v, w); which seven equations are the chief analytical results arrived at in the memoir. The memoir contains various developments in relation to the curvature of the surfaces, &c.

HONORARY MEMBERS ELECTED.

Feb. 28, 1876.	Prof. Luigi Cremona, Rome.
March 13.	Joseph Prestwich, F.R.S., Prof. of Geology at Oxford.

NEW FELLOWS.

Feb. 14, 1876.	Edward Tanner, M.A., Christ's College.
	J. N. Langley, B.A., St John's College.
March 13.	W. M. Hicks, B.A., St John's College.
	G. T. Bettany, B.A., Caius College.

INDEX OF NAMES.

VOL. II.

N.B.—The larger figures indicate separate papers; the smaller, incidental references.

SUBJECTS.

END OF VOL. II.

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